DEFENSE DOCUMENTATION CENTER

FOR

SCIENTIFIC AND TECHNICAL INFORMATION

CAMERON STATION. ALEXANDRIA. VIRGINIA

437464

Social Science Research and National Security

by

Ithiel de Sola Pool
and Others

March 5, 1963

NO OTS

SOCIAL SCIENCE RESEARCH AND NATIONAL SECURITY

by

Ithiel de Sola Pool and Others

CONTRIBUTORS

Professor Ansley J. Coale
Office of Population Research
5 Ivy Lane, Princeton University
Princeton, New Jersey

Dr. W. Phillips Davison
Council on Foreign Relations, Inc.
58 East 68th Street
New York, New York

Professor Harry Eckstein
Center of International Studies
Princeton University
Princeton, New Jersey

Dr. Klaus Knorr
Center of International Studies
Princeton University
Princeton, New Jersey

Dr. Vincent V. McRae
Office of the Special Assistant for
  Science and Technology
Executive Office Building
Washington 25, D.C.

Dr. Ithiel de Sola Pool
Center for International Studies
Massachusetts Institute of Technology
Cambridge, Massachusetts

Professor Lucian W. Pye
Center for International Studies
Massachusetts Institute of Technology
Cambridge, Massachusetts

Dr. Thomas C. Schelling
Center for International Affairs
6 Divinity Avenue
Cambridge 38, Massachusetts

Dr. Wilbur Schramm
Institute for Communications Research
Stanford University
Stanford, California

Foreword

The central question which guided and unites the separate studies in this book is stated by Ithiel Pool on page 10 of his introductory chapter: "How can a branch of social science be produced which takes upon itself a responsible concern for national security matters, and how can talented individuals from within social science be drawn into this area?" The authors assume that the first step in the deliberate creation of a branch of applied science is to describe some, at least, of the substantive tasks of the potential clientele (in this case the members of the national security establishment) and to suggest how successful research, both basic and applied, could assist them in performing these tasks. A second step is to draw the attention of the potential clientele and the potential practitioners of such a science to its potential for application in the hope that more individuals will participate in its growth. Later steps are to develop institutions dedicated to the creation of a self-conscious, self-stimulating branch of science; to supply funds; and to persuade many practitioners to practise, many clients to use, the science. In this book we address ourselves to the first two of these steps. Our prime audience is intended to include, on the one hand, those research-oriented members of the military establishment (and related governmental agencies) who engage in strategic policy planning and, on the other hand, those social scientists who have special techniques to supply tested new concepts and information needed for more effective policy planning in the future.

The studies were prepared as background materials for a research planning effort entered into by the Research Group in Psychology and the Social Sciences, Smithsonian Institution, for the Department of Defense. The work was supported, in part, by Contract Nonr 1354(08).

The late Col. P. H. Mitchell of the Office of Science, Director of Defense Research and Engineering in the Office of the Secretary of Defense, originally requested and supported this planning effort. An informal committee of the Smithsonian's Research Group selected a series of topics and commissioned each of the authors to state his personal views on the current state of social science research and on needs and opportunities for further research relevant to his topic. The committee was composed of Ithiel de Sola Pool of Massachusetts Institute of Technology, Chairman; John L. Kennedy and Klaus Knorr of Princeton University; and Charles A. H. Thomson of RAND Corporation.

No attempt has been made to consolidate into a group point of view the sometimes divergent opinions expressed by the individual authors. The writer, with the effective help of Eunice W. Shafferman, has edited the manuscript in minor matters of clarity, has imposed such uniformity of bibliographic and other conventions as the reader may find, and has introduced a few cross references. The studies remain almost as the individual authors prepared them.

The observations and conclusions should be considered in relation to the time period in which the studies were prepared, 1961-1962. And, to repeat, the opinions expressed are those of the individual authors; their opinions do not necessarily represent the opinions of their regular employers, the Smithsonian Institution, or the Department of Defense.

Charles W. Bray
Special Research Director
Research Group in Psychology
and the Social Sciences
Smithsonian Institution

March 5, 1963

Social Science Research and National Security

Table of Contents

Table of Contents (Continued)

SOME IMPLICATIONS OF THE VOLUME

Ithiel de Sola Pool

The objective of this book is to consider what social science can contribute to more effective conduct of the free world's defense effort. It does not aim to be exhaustive but rather to spot significant topics on what social science has heretofore been too little used.

Our chapters skip over those fields in which the military establishment has already made extensive use of the new technology of human behavior. Personnel selection, training, and human engineering are obvious examples. Aptitude testing and human engineering have developed and improved in large part as a result of their military use.

We skip over these familiar military applications of social science in part because they have been looked at recently in a companion report (Bray, 3)* to this one. In the fall of 1957, the Advisory Panel in Psychology and the Social Sciences of the Office of Defense Research and Engineering undertook to determine what social science research areas relevant to future military needs might be ready to make major advances in the next decade, particularly if given appropriate support. Six areas were initially identified:

Design and use of man-machine systems;
Team functions;
Human performan   abilities and limitations;
Individual decision   esses;
The adaptation of complex organizations to changing demands;
Persuasion and motivation.

Under a contract with the Smithsonian Institution, task groups were constituted in each of these areas to examine potentialities and needs.

As the Advisory Panel reviewed the outcome of its efforts it came increasingly to realize that it had inadvertently focused on one aspect of the Defense Department's problems, namely the management of its own establishment.

Ithiel de Sola Pool, PhD, is Professor of Political Science at Massachusetts Institute of Technology and active in its Center for International Studies. He is also Chairman of the Research Board of the Simulmatics Corporation of New York, N.Y. Formerly, he was a member of the Steering Group of the Advisory Panel on Psychology and the Social Sciences of the Office of the Director of Defense Research and Engineering. Currently, Dr. Pool is a member of the Air Force Scientific Advisory Board and the Advisory Board of the Naval Ordnance Test Station at China Lake.

---

*The number in parenthesis and similar numbers elsewhere refer to corresponding numbered items in the bibliographies at the end of each chapter.

With the partial exception of the report on persuasion and motivation which
concerned itself extensively with psychological warfare, all of the reports
dealt with the expansion of social science knowledge of a kind that could be
used to make the manpower of the military establishment more effective.
The Defense Department is a vast organization with complex man-machine
systems operated by millions of men in uniform and out; so it could
clearly benefit by knowing more about individual performance, team func-
tions, large organizations, decision processes, persuasion and motivation,
and man-machine systems in general. But there is also an entirely dif-
ferent domain of Defense Department problems which, the research group
recognized, had not yet been considered. To it, too, the social sciences
might make a contribution.

This other domain of problems may be roughly characterized as the
operations of the Defense Department in relation to the external world.
We had already examined the researchable problems of the Department's
internal management, but we had not yet considered just what social sci-
ence has to contribute to the Defense Department's strategic operations
against enemies, to the coordination of its operations with allies, to intel-
ligence, to weapons development, and to planning.

Neglect of potential social science contributions to the substantive
tasks of the Defense Department and focus on social science for internal
management purposes solely was characteristic not of the research group
alone. It reflected the history of actual use of social science by the De-
partment of Defense. The areas the research group had identified were
ones in which a significant beginning of social science activity on behalf of
defense had already been made and in which expansion of support could be
justified both on the basis of clear opportunities for further applications
and on the showing of past successes. With some qualifications it can be
said that up to now the Defense Department has used the social sciences
primarily to help it solve its internal management problems, not to help it
in its substantive tasks.

This is not surprising. It is the typical history of the relationship of
science to established social institutions in any field. It is always with
doubts and reservations that the manager calls in the expert. Quite rightly
he suspects that the expert, if successful, may make the subject matter so
technical that the manager will lose part of his function to the expert. The
expert may begin to take over. Expert advice can be the thin edge of a
wedge which can change an institution to the point where decisions can no
longer be made without the expert's participation. This is an uncomfortable
fact of life. It is fully illustrated in the role of the natural scientist in mili-
tary technology today. Technological advance has deprived the professional
soldier of some measure of freedom and of exclusive control in his main
strategic functions (see Janowitz, 7). To that degree it has undercut the
martial tradition and the values associated with it. The same thing has
been happening in many fields. The television technician has injected him-
self into the management of presidential campaigns. The economist has
infringed on the hunches of the businessman. This technicalization of
management is an uncomfortable fact but an inevitable one.

In the process of hesitant acceptance of such technological advance,
the first steps taken are almost always ones where the commanding gen-
eralist seeks, even if unconsciously, to circumscribe the role of the

technician to that of a facilitating and supporting function rather than to permit infringement upon his central prerogatives. This kind of resistance may be illustrated by the actions even of scientists themselves when they serve in an administrative capacity. It is notable, for instance, that university social scientists have done little research in connection with their own teaching activities. They are happy to propose expanded research on influence processes in marketing, public affairs, etc., but they have always dismissed as unfeasible the study of the influence processes in which they themselves engage in regard to their students. When they study education, they study its conduct in the primary and secondary school—not in the university. It is thus not in criticism but simply in candid observation that one must recognize that the defense establishment was early eager to accept the aid of social science on its vast management problems but less willing to concede that social scientists might have something to contribute to those decisions which constitute the crux of military planning and operations.

In general, no professional group has, in recent decades, been as receptive to science and to the contributions that science can make as has been the military. The relations of scientists and officers compare most favorably, for example, to those of scientists and industrialists. Nonetheless, we are breaking somewhat novel ground as we ask in this volume: How much has social science contributed in the past to military operations other than management? What can it contribute in the future, and what specific areas of the social sciences have something to say relevant to specific, substantive, military problems?

Sporadically social science has had impact on military activity for years, indeed for centuries. In the chapters to follow we occasionally note instances. Historians, economists, geographers, students of foreign areas, even before recognizing their fields as constituting disciplines, provided information that formed part of the equipment of military planners. Military men have necessarily been students of society. They have had to know about the availability there and production there of material resources. They have had to understand something of the dynamics of revolution, of guerrilla warfare, and of other forms of politically motivated resistance. They have always been acute observers of morale. It is an old military cliché that in warfare the psychological is to the material as 10 to 1.

In the past, commanders have not generally attached professional social scientists to their staffs to inform them about social science matters. They have quite rightly felt that they themselves understood such matters as well as any long-haired theoretician. They did not believe that a special technology existed for learning about human behavior, and as we have said, until recently they were certainly right. Commanders did indeed pick the brains of nonmilitary scholars. They read their books. They studied their writings and theories in the military academies. They talked to them. But that kind of informal acquisition of knowledge was enough, since the commander himself could by such means become as expert as any technician he might hire.

The question before us is whether in the social science field, as in so many others, that situation is changing. In what fields is there coming into being a technology of human behavior that can provide highly important

information of a character so technical and so difficult to assemble that it requires specialists to deal with it?*

That is in part what the studies in this volume examine. The answers are not uniform, for the situation varies from field to field. Vincent McRae points to the development of a considerable technical expertise in gaming as a planning procedure. Drs. Eckstein and Coale point to shortages of specialists working in some areas that could otherwise make contributions to military planning. Klaus Knorr cites the work of Alexander George on intelligence inferences from propaganda material and of Abram Bergson on the estimation of the Soviet gross national product as examples of technical social science work which has contributed to intelligence. Wilbur Schramm cites the strategic bombing surveys and studies of the different psychological effects on troops of high explosive bombing, Napalm, and other weapons as examples of social science studies of weapons effects. Drs. Pye and Davison identify contributions which the area specialist and public opinion surveyer can make; such area surveys go beyond what has always been useful in travelers' tales a la Herodotus or Marco Polo, which a generalist could easily assimilate. It is perhaps in the field of strategy that a new scientific profession, whether one chooses to call it social science or not, is clearly emerging. We shall comment on that field in particular below.

In one field our job has been done for us. Hitch and McKean, in The Economics of Defense in the Nuclear Age (5), have already done for economics what each of our chapters tries to do for other areas of the social sciences. That volume had just appeared when our work began. It freed us from the necessity of dealing with what would otherwise have been a central problem: the allocation of scarce resources to achieve desired security goals.

Our plan of organization is slightly different from that which Hitch adopted. He picked a discipline, economics, and looked at its contribution to military activities. We picked military functions and asked what social sciences might contribute to each of them. Nonetheless, we have skipped over contributions by economics. One military function is logistics, but it is an activity to which the discipline of economics above all makes a contribution. We therefore do not deal with it. Contributions of economics to fields such as intelligence and strategy are but lightly alluded to in this volume.

It might be well here to note briefly some of the other important military topics we leave out by our deliberate exclusion of economics. Hitch and McKean concern themselves, among other things, with the relationship between defense spending in the aggregate and the gross national product. They ask: How big can the defense budget be, with and without serious economic consequences? What effect does the size of the defense budget have on the national economy? How do the answers to the preceding questions differ between the United States and other major powers. These are clearly questions which concern the Defense Department in an era wherein

*A sub-panel of the President's Science Advisory Committee recently examined this question. For an excellent review of what the behavioral sciences have learned that is not obvious to common sense, see Miller (9).

it is technically easy to design weapon systems, the major drawback of
which is that they would cost several times the gross national product
per year.

Hitch and McKean also concern themselves with the measurement of
the efficiency of alternative allocations of defense resources. Where re-
sources are scarce, cost-effectiveness becomes a critical consideration
in choosing a weapon system or a support system. Hitch and McKean con-
sider such problems as the following: What, for example, would be the
optimum balance between higher speed or lower speed transport planes,
longer range or shorter range ones, in each case recognizing that the
added capability is bought at a cost which may reduce the number of planes
obtainable? What is the value of a larger number of cruder missiles now
or a smaller number now but with more refined and advanced ones com-
ing along in larger numbers at some subsequent date? Hitch and McKean
consider also the economies and waste of duplication in research and in
development activities, the cost and economies of division of labor among
allies, the economics of post-attack recuperation, and a number of other
rather special problems.

The above problems, then, dealt with by Hitch and McKean, are among
those excluded from this volume. Thus with logistics, management, per-
sonnel, training, and human engineering problems being excluded from our
purview, there remain for our consideration a number of military tasks in
which social science might possibly be of greater use than it is at present.
These include strategic planning, weapon system selection, intelligence,
guerrilla warfare, and such external relations of the defense establishment
as civil-military relations, military government, military assistance, and
interallied relations. It was in these broad areas that we looked for prob-
lems to which the social sciences as they now exist might provide contri-
butions.

To identify these problems and social science work which might con-
tribute to their solution, the Smithsonian Research Group in Psychology
and the Social Sciences commissioned a number of papers. A selection of
these constitutes the chapters which follow.

The Research Group has made no attempt to resolve differences of
opinion between the authors or to formulate a group position. Except for
minor editorial review for clarity, each paper, including this one, is pub-
lished as its author wrote it. Each author presents his individual sugges-
tions as to how social science might help in the national security effort.
These papers are published here without group endorsement for the pur-
pose of stimulating relevant research both privately and with public support.
They are published also in the hope that they may help persons concerned
with the nation's defense to understand better what social science may be
able to contribute to their work.

SOCIAL SCIENCE AND DEFENSE IN THE NUCLEAR AGE

The invention of nuclear weapons, and now ballistic missiles, has
done more to change warfare in a period of less than two decades than has
been done in all the history of mankind up to 1945. The incredible des-
tructiveness of nuclear weapons and the speed and automaticity of

modern delivery systems have transformed both the role and uses of combatant forces. Throughout the history of mankind military strength has depended on the mobilization and motivation of large bodies of trained fighting men, ready to risk their own lives and to take action against visible human opponents. In a nuclear war it is at least conceivable that the outcome would be determined before any soldier became engaged in any act of combat with another human being. Morale and training might in some circumstances have no effect on the outcome. The technical processes of launching missiles have lost all resemblance to the familiar acts by which living creatures fight each other.

It is ironical that exactly at the historic moment when technology seemed to undermine the role of the human fighting man and appeared to replace him in part by automated machine systems, social science appeared ready to make important contributions to defense operations. The social sciences seem to be coming into their own as effective instruments of policy in precisely the same decade that the natural scientists have reduced the human element in strategic combat. Social science, if it had existed, could clearly have been of enormous use to a Caesar or a Napoleon operating over large areas with great masses of human fighters. But social science did not then exist. It happened to emerge coincident with intercontinental ballistic missiles.

That might lead one to a negative conclusion about the potential usefulness of the new instrument, social science. It may seem to have come too little too late. And if the effect of modern military technology were always to replace the soldier by the missile, then that conclusion would be valid. It turns out, however, that the real effect of automated procedures in warfare, as in all fields, is not to minimize the role of the human element, but to increase it. The way in which the human factor operates changes, but it becomes ever more complex and critical. .

Note, for example, the problems of command and control. It may perhaps turn out, should a strategic nuclear war ever occur, that there will be no need to think about the morale of millions of fighting men (though even that is not a foregone conclusion), but nothing would be more critical in that situation than the behavior patterns and efficiency of that small group of men who control the buttons and the messages which affect them. An understanding of the human element, though a changed one, is important for the safe operation even of strategic forces.

We should perhaps emphasize that if the human element is vital in strategic warfare, it is even more so in most wars that actually occur in this nuclear era, which, as Eckstein and Pye point out below, are small wars.

Yet the always imminent, but hopefully never actual, all-out strategic combat is a dominant factor in the world military situation. The situation for strategic-war planning has changed not only in the possible elimination of man to man combat under one set of important circumstances but also in the fact that the main strategic weapons have become so horrible that it is not the intention that they should ever be used. To deter, not to prevail, has become the purpose of at least many major weapon systems. We say this without intending to introduce a false distinction between deterrent systems and combat effective systems. A system which

would fail in combat will not deter. Nonetheless, a change in purpose of weapons has occurred. Their primary purpose has become to preserve the peace. Weapons are designed so that they could win but with the goal that they should not be used. That represents an enormous change. Never before in history have military men and statesmen been convinced that war may prove to be unusable as an instrument of policy; that, despite all posturing of forces and minor incidents, use of one's strongest weapons may by deliberate choice be excluded. Never before have military men had to consider seriously whether their primary function might be a bargaining and diplomatic one.

So it turns out, contrary to what one might expect. that the potential contribution of social science is made greater than ever by the new military technology because the critical human functions are so novel. Commanding generals who well understand the art of keeping fighting men in a state of discipline and alert may be far less at home in political bargaining. They may also feel unsure in managing the elaborate industrial maintenance operation of a missile site. They may be even less confident about the ways to get high creativity out of scientists or productivity out of computer programers and radar technicians. The human problems of modern weaponry are vast and unfamiliar ones. They are the problems of gigantic organizations, of intercultural operations, and of organizations with goals additional to combat.

Thus the two trends we noted before, the technological revolution in weapons and the change in their primary purpose from use to deterrence, both increase the demands upon social science.

Deterrence, as has often been commented, is essentially a psychological matter. The vast and destructive weapons which are part of a deterrent system do not by themselves deter. This is pointed out in the chapter by Wilbur Schramm. As Thomas Schelling suggests below, the role of weapons is to communicate a message in a bargaining process. Many things besides the true facts of weapons deployment and capability affect whether the message will be conveyed and in what form. We do not wish to overstate the case. Little can be done to convey a deterrent message if the physical deterrent does not exist. The potentialities for pure bluff in international affairs are decidedly limited. They are not non-existent, particularly for countries unlike our own which can shroud their society in a fog of secrecy. Note the comments by Wilbur Schramm on the advantages which a closed society has in dissembling its intent even if not always its capabilities. Note also the sage precepts he quotes from Bernard Brodie about the techniques for effective disclosures of one's weapons.

Indeed at the moment of writing we are just coming out of an era of successful bluff, perpetrated by the Soviet Union. By dramatic space shots, nuclear tests, and other devices the Soviets led many persons in the late 1950's to fear that there was going to be a missile gap in their favor. And the resultant overestimation of their strength may well have bought them substantial deterrence of the West in a number of situations. We may have been more cautious and moderate about Berlin and Eastern Europe in general than we had any need to be. But note the limits of what bluff could buy. Even the totalitarian Soviet system was not able to keep our estimates off base for more than a year or so. If anything, today (December

1962), their past bluff may harm them, for in reaction to our underestimation of their past weakness we are likely to err, insofar as our estimates do err at all, in the direction of discounting their prospects.

So let us not underrate the importance of the facts as conveyors of deterrent messages. But let us not overrate them either. Facts do not talk for themselves. Neither do weapon postures. They are meaningful only insofar as they are known, credible, and understood. The arts of communication have a significant contribution to make to deterrence.

The social sciences have a good deal to say about how to reach men of particular cultures, ideologies, and personalities. Schramm cites the evidence that material from a distrusted source is more effective when overheard rather than sent directly, and if it sets up cognitive imbalance. And a second example may be noted from Schramm: Press reporting of other nations is so poor nearly everywhere in the world as to leave people with grossly inaccurate perceptions of the purposes and policies of others. Yet knowing the feelings and desires of an audience is the first step to communicating to them. Two of our authors—Schelling and Schramm—note that, hard as it is to have accurate information on foreign actions, it is even more difficult to know the facts on foreign attitudes.

Attitudes are harder to read and report than overt events, and they are easier to misinterpret. How much weight shall be given to the emotions of a few thousand ban-the-bomb demonstrators? What do they represent in the rest of a population? What kinds of hidden feelings might emerge in a moment of crisis? Such attitudes, on which our security depends, are so badly reported now that independently several authors in this volume give high priority to social science research designed simply to probe the facts about worldwide attitudes toward weapon postures. Schelling cites the kinds of surveys Daniel Lerner has done in Western Europe on feelings about the Polaris system and the reliability of the American guarantee.

Schelling, Schramm, and Davison all endorse the kinds of surveys conducted by the United States Information Agency and revealed in the election campaign of 1960 under the misleading rubric, "the prestige polls". These extraordinarily valuable studies, which unfortunately now appear to have been somewhat curtailed, gave irrefutable evidence of a fact most American observers found difficult to recognize in 1960, that most of the world saw the Soviet Union as militarily stronger than we. Understanding such facts about the beliefs of our allies is essential to smooth working in alliance relations—Davison stresses the value of allies studying each other as well as the enemy. Understanding whether we or the Soviets are viewed as stronger is essential also if one is to make one's deterrent posture credible. What seems to us a stand of unmistakable significance in the light of our belief in our strength may seem quite silly and meaningless to someone who does not recognize that we have that strength. Only objective surveys can give us the facts on what others think.

Note also an interesting result of gaming. In games, a military posture believed by those who took it to be an unmistakable indicator of intent, is often unreadable by the other side. The other side sees only the beginnings of moves whose planned development are for normal military reasons kept secret. The full scope and rationale of the plans are known to the

initiators who therefore fail to consider how alternative interpretations
could be given the preliminary facts. Moves are initiated without adequate
attention being paid to what the isolated initial acts may convey to those
who are not privy to the full plan.

So strategic war planning puts heavy demands upon the social sciences.
A large part of contemporary military activity, however, has nothing to do
with nuclear strategic forces other than in existing under their umbrella.
A great part of contemporary military effort goes into situations where the
primary requirements are good political relations with foreign nationals.
(Cf. Davison's chapter on alliance relations.) The job of the American of-
ficer overseas is partly that of diplomat, partly that of policeman, partly
that of propagandist, partly that of technical assistance worker, and partly
that of social service worker.

No point perhaps has been touched on by as many of our authors as
the fact that contemporary military policy must be as much concerned with
the political effects of military postures short of war as with force itself.
Davison, Schramm, Eckstein, Schelling, and McRae all take note of the
political effects of weapons, weapon tests, deployment, etc., on negotia-
tions and crises. Military planning cannot be concerned only with victory
in combat. It must be deeply concerned with the political consequences of
weapons in many different lands and cultures. The world has become so
small and the inhibitions on use of force so complex that every military
move must be evaluated by what it means to allies, neutrals, and foes in a
hundred different countries, any of whom may be listening to anything the
United States says or does to any other country.

In many places the military job can only be accomplished by a process
of nation building. Lucian Pye raises the critical question of whether our
military aid programs have actually been stimulators or depressants to
modernization and development. Clearly, if they are to be successful they
must be stimulators. In countries at the edge or over the edge of insur-
gency, an American officer working in close daily relations with people
from the local culture can foresee an end to his job only when he has
helped the local people establish all the prerequisites of stable nationhood.
These include an adequate communication system, a growing economy,
faith in progress, a set of political parties and pressure groups working
toward national goals, .. disciplined civil service, a sound currency, liter-
acy, an education system, an honest government, .nd a modern ideology.
Eckstein and Pye agree that any military plann.   n an area of insurgency
who thinks only about how to spot a few peasant   .ds hiding on a forest-
covered mountain is likely to be fighting an endl 's and, ultimately, a los-
ing battle. Success in counter-insurgency outflanks a stalemate in the
field by concentrating on actions which will in two to five years' time es-
tablish stable communities in a progressing nation, with the surviving
guerrillas quietly returned from their hide-outs.

It is not new for soldiers to be nation builders and establishers of
governments. That has always been one of their roles. What is new is that
they must now do these things with much less freedom to use the traditional
military instrumentalities of force. In the past, conquerors established
governments backed by their bayonets. Today the military assistance ad-
viser must operate with every consideration to the acute sensitivities of
alerted nationalism in a work in which brute force is both not tolerated

and too dangerous to try. So once again we see, ironically, that modern
weapons have made the human element in the conduct of military policy
more complex than ever.

Hence we conclude that in an age of automatic weapons military men
must deal with more social relations problems, no* fewer. And more, the
human dimensions have become so complex that intuition alone is no longer
capable of dealing with them. Science is called for.

CAN THE SOCIAL SCIENCES MEET THE CHALLENGE?

The challenge is clear; the need is great. What is not clear is
whether the social sciences will contribute in proportion to the need.

Comparatively few social scientists in the past have addressed them-
selves to defense problems. Like all specialists they have been mobil-
ized in wartime. Most of the World War II generation of social scientists
had experience in psychological warfare, military government, target
planning, or intelligence research. But after the war there was a rapid
return to normalcy. Their skills no longer seemed needed; their interests
quickly shifted. Only a handful maintained a continuing interest in prob-
lems of a military character, and the few that did were soon discouraged
by the Government's lack of interest in maintaining its capabilities. This
was strikingly apparent in the psychological warfare field, where the mem-
bers of what once was a rather large corps of highly skilled and interested
persons have gradually lost touch with the military establishment. In the
universities the subject has become an almost dead one.

In small part, the weakness of militarily-oriented social science may
be attributed to an unmilitary and even anti-military sentiment among some
social scientists. They tended to be personally liberal in ideology. Sub-
stantial areas of sociology and related disciplines can be regarded as secu-
larized forms of earlier social work and missionary activities. An exami-
nation of the life history of many social scientists will reveal either
parental or early interests of a religious or reforming kind. To all of
these elements might be added a certain ivory tower viewpoint. Many so-
cial scientists have seen their discipline as essentially a form of social
criticism. As such they have resisted identification with the policy-making
process in established institutions, be they public or private. Many social
scientists have shied away from helping solve the problems of industry,
defense, or civilian branches of government.

Yet it would be wrong to regard these attitudes of some social scien-
tists as the major reason for the absence of much good military-oriented
social science. A much more important reason has been the lack of de-
mand by the military establishment. Had they been urged, many social
scientists would have responded.

I address myself here to consideration of how this situation can be
changed. How can a branch of social science be produced which takes upon
itself a responsible concern for national security matters, and how can
talented individuals from within social science be drawn into this area?
That this is feasible and deserves to be attempted is a thesis underlying
the efforts of the committee that produced this volume. Implicit in our

effort was the assumption that organizational and fiscal measures can have
some effect and that the direction of movement of a science can in part,
even if only in part, be influenced by provision of financial support. The
authors of the studies in this volume do not agree in full among them-
selves upon what conditions are optimal for development of a discipline
or at least of their own disciplines. The issue is drawn most sharply be-
tween Ansley Coale and some of the other authors. Where Coale sees
project research as diverting his discipline of demography from the es-
sential task of training cadres through fellowships, others among us would
argue that the demands made for results through project research would
in turn result in recruitment and thus stimulate the flow of further support
into training. Rather than arguing this specific policy issue, let us con-
sider what can be said on the basis f history about the conditions for the
growth of a new and desired discipline.

The conditions are partially endogenous to the science, partially exo-
genous to it. There must be a nucleus of ideas, people, and techniques, or
no amount of external support will create them. If external needs alone
were enough, we would have had science many times over in the history of
man's struggle with nature. But given certain essential endogenous con-
ditions, favorable exogenous factors may make an enormous difference in
where and when science develops. It is certainly no accident that in the
modern world the great centers of science are also the great centers of
industrial development, wealth, and political power. Although, in principle,
the ideas which form physics or sociology may be universal, and men of
genius may be born everywhere, the great advances in research which
produce many Nobel Prize winners and their equivalents in the social sci-
ences come in a handful of centers in a handful of societies, and these ones
with very uniform social characteristics.

The forces, endogenous and exogenous alike, that help account for the
growth and development of sciences, operate on two sets of factors which
for want of better names we may call institutional and intellectual. With
two dichotomies, endogenous and exogenous, institutional and intellectual,
we have a four-fold classification of causal factors. An example of an
endogenous institutional stimulus to science is the atmosphere and admin-
istration of a well-run university. Conversely an institutional factor within
a discipline which may hamper its growth is the failure of the universities
to recognize a science. In England, Oxford and Cambridge have not yet
been willing to establish chairs in sociology, and that fact has placed upon
British sociology the stamp of industrial applied research on the one hand
and of the red-brick universities on the other. Only recently have the
first moves been made to recognize behavioral science in the always con-
servative major British universities, and, one may expect, with substantial
intellectual effects.

Another institutional condition within a discipline which leads to rapid
growth of a science is a critical size. As important at the time of Plato's
Academy or the medieval universities as today is the existence of an as-
semblage of individuals with common interests, in a place, and with ade-
quate facilities and reasonable freedom to think. Once the practitioners of
a science find themselves interacting with others like themselves the
growth of the science tends to follow a logistic growth curve. The scien-
tists generate ideas that provide stimulus to the thought of their colleagues.
A science may explode. As I. I. Rabi (13) has recently shown, American

physics is an example in point. A whole generation of American physicists were trained in England, Switzerland, and Germany in the 1920's, thanks largely to National Research Council fellowships. They returned to the United States and in the 1930's trained a large successive generation so that, when the war came, American physics led the world, and a cadre existed able to produce the great revolution in military technology of the war years. A critical mass of physicists that had been produced in the late 1920's created a critical mass of fissionable material at Stagg Field less than two decades later.

The last example is one not only of the effects of endogenous institutional factors but also of exogenous. Without the National Research Council fellowships, there might never have been the change in the internal situation in physics.

Princes, educational institutions, governments throughout the centuries have by provision of funds and facilities often determined the direction of scientific growth. In a less friendly environment, Columbus might easily have been denied his ships, Galileo his chair. Scientists today worry that the pouring of funds into travel to the moon may distort science from promising directions of effort into ones of little theoretical interest. But for good or evil, it cannot be denied that decisions on funding will have a profound effect upon the direction of science.

The intellectual stimuli to the growth of science, both endogenous and exogenous, are much harder to analyze but are at least equally important. As Schelling notes below: "Research will get done mainly because there are qualified people who can be interested in it. Financial support can help, but moral support, suggestion, and the assurance of a receptive audience are as important as funds. Identifying fruitful lines of inquiry, particularly those that have intellectual fascination, is at least as important as developing a 'program'."

In general, it may be said that at the early stages of the development of a science the intellectual stimuli tend to be largely exogenous. Darwin borrowed his seminal idea of the struggle for survival from the social scientist Malthus. Hobbes based his political theory on a borrowed model from the mechanical physics of his day. Freud described the human mind by an almost hydraulic model of forces and flows. Computer technicians today find that they have to borrow the purposive language of human thought and action, talking of computer memory and system goals for example. Information theory got its first notions from cryptography, though it could equally well have borrowed them from thermodynamics. Strategic analysts are perhaps influenced by game theory. To what degree we shall further consider below. So it goes at the beginning of almost every discipline. The seminal idea usually is borrowed from some entirely different field.

An outstanding contemporary example of how one discipline borrows from another is the influence of system analysis, statistical decision theory, economics, and game theory upon strategic analysis. Critics of current American strategy such as P. M. S. Blackett (2) accuse such strategic thinkers as Herman Kahn, Albert Wohlstetter, Henry Kissinger, Thomas Schelling, Oskar Morgenstern, and others of using game theory about matters too important and too complex for it. A common retort to

this criticism is that in fact no strategic analyst has used game theory. The retort has much to justify it. It would be hard to find an example of a major issue which has been analyzed by use of the mathematics of game theory. The facts sustain the retort. Yet there is a relation of game theory to modern strategic thinking that a historian of thought would have to note. Game theoretic notions were in the air in the years that saw the evolution of a quite new kind of strategic thinking. The mathematics in Von Neumann and Morgenstern (15) had an influence on manuscripts which contain not a single equation and whose conclusions could not be mathematically demonstrated by any known formal process. The borrowing has been analogical. Some attempts have been made to borrow from game theory in a mechanical and formalistic way but without success. The important originators in modern strategic thinking have been influenced in more subtle ways.

One line of influence may be seen in the work of Schelling, who has contributed to the theory of non-zero-sum games. But Schelling's contribution has been above all to underscore how inadequate the mathematics of the zero-sum theory is to a situation where bargaining takes place among players who have limited information available to them and that changing with time. And even the contributions of Schelling to game theory have been used by Schelling the strategist and arms control specialist in ways that are analogical and heuristic. Writers on strategy often use game theoretic language, but seldom or never solve a set of game theory equations or matrices. Note that in this volume no author has used game theory as such. Schelling has suggested the relevance of game theoretic ideas to the problem of inspection for violations of arms-control agreements. McRae who writes about gaming pays no attention at all to game theory. It is referred to more extensively in several of the other studies in which it appears only in obiter dicta having nothing to do with the technical subject of the paper. It is clearly not a major element in contemporary strategic analysis. Yet a sensitization to the kinds of dilemmas involved in bargaining situations and a categorization of these into major classes for rigorous thinking were undoubtedly aided by the presence in the environment of concurrent discussion of some highly formalized and non-military game problems; note the classical "prisoners' dilemma."

Closely related to the evolution of game theory has been the evolution of statistical decision theory and with it the Bayesian approach to statistics. Strategic decisions are, after all, choices made in a state of uncertainty and with limited information. That is what statistical decision theory is all about. Good strategic analysis is also quantitative. It depends critically upon the facts. Whether fall-out shelters will save x million lives per y billion dollars invested in them is a function of numbers concerning megatonnage delivered, at what points aimed, with what circular error, and under what conditions of wind and population distribution. Such questions cannot be answered by reference to broad principles. Our ability to deal quantitatively with complex systems of this sort and to make estimates of their operation has been the product of many developments in computers, economics, statistics, and operations research—all of which hang together in something called the system approach. Game theory is one of the least used parts of that package. Yet without its evolution we would be thinking far less clearly about the purposive element of systems, strategic or other. Today, strategic analysis, in its adolescence, may begin to forget about its parentage as it sets out to do its job in the world. But it would not be a

discipline today without the historic influence of all these other disciplines upon it.

At later stages in the development of a science, endogenous intellectual stimuli become much more powerful than outside ones. A field develops insights and models of its own which in turn generate new ideas. The private symbolism of the field becomes too complex to incorporate simple notions of outsiders, and its own equipment becomes sufficiently powerful to make the field's growth self-sustaining. Thus it is much harder to explain developments in the physical sciences after the nineteenth century by looking at what was going on in the intellectual environment of the society as a whole than it is to so explain their earlier development. Social and cultural determinism gives way to an internal determinism of the discipline itself.

The contrast may be fruitfully illustrated by comparing the great insights which a social historian such as Edgar Zilsel (17) could bring to bear upon the growth of science in the age of Galileo with the fruitlessness of trying to explain trends in modern physics in the same way. Zilsel demonstrated how the emergence of social contact between intellectuals on the one hand and tradesmen and artisans on the other—men who at an earlier period had been socially isolated from each other—brought the disciplined intellect of the scholars to play upon currently acute problems in mining, medicine, and navigation. Galileo was motivated by the new ethos of the commercial cities in which he lived to address   self to such problems as the swing of a pendulum, the trajectory   cannon ball, or the flow of water in a pipe, subjects which at an earlier age an intellectual of his power would have disdained to consider. He and his scientific contemporaries were able to bring rigor of analysis to bear on simple facts that artisans had long observed. But the insights of the artisan no longer provide fuel for science in the twentieth century.

The social sciences are now where physical science was in the sixteenth century. The social scientist brings the benefits of formalization and systemization to the experience-derived wisdom of the operator from whom in turn the social scientist learns a great deal about the real world. Economics, it is true, has an autonomous theory of its own which will probably determine its directions of further growth. So in that one social science borrowing from business wisdom will probably provide less stimulus to discovery than will theory. The same is true for a few of the more advanced areas of psychology. But with a few exceptions, the social sciences are at the point at which the endogenous forces are still much weaker determinants of the direction of growth than the exogenous ones. That being the case, the mere mobilization of some of the pool of disciplined intellectual capability of academic persons by getting them to address some of their energies toward the practical problems besetting administrators in business, government, the armed forces, or other fields is likely to produce a dramatic and important pay-off not only to practical affairs but also to the social sciences. The problem is still that of getting thinkers to operate systematically and imaginatively on the practical problems of the practitioners.

As a result, many social science studies may at first glance seem more like staff studies than like science. An intelligent, thoughtful, academic man is asked to devote his attention to a practical problem just as

in the sixteenth century learned men were asked to help the miners, soldiers, and mariners. There is, however, a difference, as Davison notes, between the social science study of an operational problem and a staff study. The difference is in the degree of interest in generalization. Both start at the same place, with a practical problem to be solved. The staff study stops with advice for a particular time and place. The social science study may contain such advice, but its author is also interested in generalization. What differentiated Galileo from a gunnery teacher in his day was not the problem with which each dealt. They were both concerned with estimating trajectories of cannon balls. But Galileo let his inquiry go as far afield as needed to arrive at explanatory principles. The gunnery teacher was answering the same questions by practical trial and rule of thumb, and for the short run sometimes answering more usefully than the scientist.

Similarly, today, staff studies done in the Department of Defense cover, in many cases, the identical topics that some of our authors have suggested for social science research. This is as it should be. The Department needs both approaches and should support both. In the short run each will make some contribution; on any given occasion either may turn out to make the greater contribution. In the long run the studies that seek to add to science are the seminal ones. They cannot be distinguished by the statement of the problems, but only by their attack upon them.

Many topics for study are recommended in the various chapters of this book. Schelling suggests that we examine the process of negotiation. Davison suggests looking into such matters as the peacetime frictions produced by the presence of foreign military personnel at overseas bases or into the difficulties which arise in joint international maneuvers. Eckstein tells us we need more case studies of modest revolutions. Knorr wants research on the functioning of intelligence organizations. These are suggestions for case studies, suggestions for having bright and well-trained men look at the facts and come up with advice. Such inquiries in themselves are not necessarily science, but that is where science begins. Out of such studies of operational problems will come the more theoretical formulations which in turn acquire a life of their own. Such studies call forth the scientific djin whose full potency no one can guess in advance.

If this is a valid description of the stage at which the social sciences are today, then it seems clear that a major step forward in the development of a body of knowledge of security relevance can be achieved by the defense establishment if it chooses to put an adequate level of consistent and long-term support behind such social researchers as are willing to turn their attention to its problems.

Perhaps the reader may question how well the structure of the military establishment lends itself to establishing such a supportive relationship with social sciences. The answer seems to be—very well. Among the exogenous factors of frequent importance in the development of a science is the existence of some applied profession, the members of which become the clientele of the scientists. The soldiers, navigators, miners, physicians, and artists of the sixteenth and seventeenth centuries were such a clientele for their contemporary scientists. Physicians are today an obvious example of professionals who depend on science. They are the clients of physiologists, pharmacologists, biochemists, bacteriologists, etc.

A consuming profession is an important asset for the development of a science. In the market for science, as in any market, there is discourse between buyers and sellers for many reasons. For one thing, it facilitates the dissemination of scientific findings. Scientists normally talk a language which the general public cannot understand. The applied profession creates a group of men who talk the same language as the scientist, who can appreciate his product, and who share with him a good deal of common training. Physicians and biologists, for example, work side by side in the same institutions with levels of training that are approximately equal. They understand each other's jargon. The physician transmits the biologist's results to society.

Medicine is an unusually clear-cut instance of a client profession for science. To some degree, such a profession exists for most expanding sciences. A science which is of service only to the general public as a whole without intermediaries is likely to be feeble. Engineers play for many physical sciences the role that physicians do for biology. Business planners and national planners play it for economics; educators and personnel men, for test psychology; social workers, for some of sociology. In each case the training of the applied practitioner is close to and includes a considerable amount of the scientific discipline to which he is related. In each case a great deal of intercourse takes place between the pure scientist and the practitioner. In each case a large and relatively well financed profession of practitioners is able to make support available to scientists who will work on problems which the practitioners regard as important.

Clearly the military constitute an applied profession which can be the client for satellite scientific disciplines. Increasingly the military are that to the natural sciences. Increasingly officers e being sent for advanced technical training to learn the language of th scientists who are thei consultants and the researchers in their laboratories. Increasingly officers become the sponsors for scientific effort. Increasingly, as noted above, there is a good personal relationship between scientists working on military problems and the officers directly responsible. Within each military service there is coming to be a more or less well-recognized specialty called Research and Development. The university-trained officers with that specialty who manage research in service laboratories or monitor contracts are coming to be entirely at home in the company of research scientists.

This has not yet happened much in the social sciences. Bridge arrangements between social scientists and officers scarcely exist. Until recently relatively few officers were trained in the social sciences. The in-service social science laboratories are few, poorly financed, and for the most past limited to special fields such as personnel selection, perception, man-machine systems, and so forth. Correspondingly, close personal relations between officers and social scientists are rare.

But this state of affairs need not prevail. The defense establishment can make itself a client, stimulating the behavioral sciences and using them—to mutual advantage. In this volume numerous ways are noted whereby social science, even as it exists today, can help the military planner.

In effect, what we have been saying is that social science needs a kind of eng'neering to go with it. Scientists and engineers do overlapping work. Engineers sometimes make scientific discoveries, and scientists often make inventions. It is often hard to tell th job of one from the other, and yet in the natural sciences an irstitutional line does exist. The social sciences have not drawn this line. Yet engineering does exist within them often dressed as social science.

The social scientist has taken the natural scientist as his ego-ideal; and wh n he is really acting as an engineer, he is hamstrung by having the wrong image of himself. This is one of the factors explaining the somewhat ivory tower approach of social scientists to which we ref rred above. Indeed, more often than not the social scientist is doing engineering, though he tries to describe it in the philosophical categories of science.

Take political science. It is the one social science which puts science in its name, and yet it is in fact almost totally a kind of engineering. It uses scientific generalizations about human behavior, arrived at by psychologists, sociologists, anthropologists, and economists. It uses them to answer such questions as how to make a government more stable, how to make a parliament more representative, how to make a treaty effective, how to prevent a revolution, how to deter an aggressor, and so on. These action problems are engineering problems involving applications of known principles of human behavior derived from the sister disciplines in behavioral science.

The fact that there are principles available to be applied signifies that there is a basic science of human behavior as well as an applied one. The point we are making here concerns not the existence of each of these but the relative magnitude of each in the daily activities of social scientists. Most of what they do is applied problem solving. In the present volume Davison has discussed the social science contribution to making alliances more cohesive. Coale has discussed how to estimate how many troops an underdeveloped economy can sustain. Pye has discussed the problem of training persons from such cultures in the arts of modern warfare. Schramm has indicated our need for a better understanding of the deterring consequences of alternative weapon postures and of communication systems about postures. These are action problems.

As the social sciences become more effectively linked to the policy process, whether in the area of defense or in other areas, they are producing a cadre wi h a new professional role like that of the engineers who work side by side with the more theoretical social scientists. The function and the activity already exist. Almost all social scientists, both inside and outside of university life, are fulfilling the new role. But a proud image of that role does not exist. Few social scientists would be happy to admit to being problem-solvers rather than scientific discoverers. What is needed is a prouder image of the role and along with that a great increase in numbers, improvement of morale, and clarity of purpose of the cadre fulfilling it. Such individuals form a vital transmission link between those who are working on the frontiers of science and its users. The appliers provide links in both directions. Sharing the language, education, and background of the research scientists, the appliers are able to bring the insights of the research scientists into the policy process. Sharing the purposes, practical orientation, and immersion in current problems

of the policy-maker, they direct scientific effort into rel ant directions.
This is not a second-class role. The engineer, the physician, or the law-
ver does not regard his role as a second-class role. Neither is there
reason for the applied social scientist to be apologetic.

Perhaps the outstanding example of a group of social scientists work-
ing on policy problems is found within the RAND Corporation. There a
massive scientific effort has been linked with at least a certain amount of
work of a highly concrete directive character with regard to defense. A
great deal of America's present national military posture can fairly be
attributed to the influence of social and related analysis made there.

An applied social science function has developed furthest in economics
and psychology. In personnel selection and training, psychologists are
deep in concrete operations. Or take as an illustration the President's
Council of Economic Advisors. Their job is not the advancement of eco-
nomic theory. Their job is the application of economic knowledge to public
policy. They cannot end their reports with the comfortable caveats so
prevalent in the social sciences that their assertions are true only ceteris
paribus or that more research is needed. They cannot assume that the
environment will remain constant. They must advise the President as to
what the actual trend of employment will be or what the actual consequences
of a change in interest rates or of a tax cut will be. Being in the role of
applied professional men rather than that of academic scientists, they must
make specific predictions however much they may admit to uncertainty.
No more than the man who builds a bridge can they relax with the com-
fortable evasion so common among social scientists that they are only de-
veloping models, not making predictions.

Knorr. in his discussion below, contrasts the social scientist, who re-
sorts to abstraction rather than to prediction of concrete social behavior
in all its complex detail, with the intelligence officer. He takes note of the
advantages and disadvantages of each in reliability on the one hand and use-
fulness on the other. But Knorr also notes the aspiration of the social sci-
ences to move toward concrete prediction. He considers the mixed record
of the economists in attempting to do so. He mentions computer simula-
tion as containing some promise of making the vast number of variables in
social reality tractible to simultaneous concrete representation.

Operations researchers have often been more willing to predict spe-
cific behavior than have social scientists. They have more of an engineer-
ing tradition and also are inclined to try to look at whole systems in their
interaction. As social scientists mature to the point of trying to give en-
gineering advice on specific actions they will find the line between them-
selves and operations researchers disappearing. While operations re-
searchers have not hesitated to try to solve specific problems with precise
quantitative predictions, they have tended to be simplistic, to say the
least, in the assumptions they introduced about human behavior. Their
successes prove only how powerful measurements can be even in the pres-
ence of vastly oversimplified assumptions. Social scientists on the other
hand have tackled the same sorts of problems without usually being will-
ing to test themselves against measurement. But as they have become
more daring and particularly in those fields such as economics with strong
quantitative traditions, they have come together with the operations re-
searchers to the point of becoming virtually indistinguishable. Operations

research is one of those exogenous intellectual stimuli to which we referred above, a stimulus which is transforming social science and making social science engineering into a reality.

But the full emergence of the social scientist into concrete planning is not likely to come by the simple application of operations research techniques. Operations researchers have perhaps been helped by their fundamental ignorance of human behavior and their oversimplification of it. They have as a result been less inhibited in forcing reality into simple models that have sufficient elegance to permit methematical solution. These models also have sufficient unreality to make social scientists uncomfortable.

It is simulation, both by computers and humans, rather than straight mathematical operations research that is a glimmer on the horizon suggesting that perhaps the vast complexity of human behavior can begin to be handled in a way that will avoid ceteris paribus evasions.

McRae's study reviews primarily human gaming though with some reference to computer simulation. It is interesting to note that gaming as a new approach to understanding human behavior has emerged largely because the defense establishment's need for prediction of human behavior has been strong enough to force the growth of a technique which does not restrict itself to one variable at a time but takes account of enough interactions among variables to represent the complexity of reality. The total amount of gaming sponsored by defense agencies, as noted by McRae, is indeed staggering.

Yet even gaming, which uses the insights and judgments of human beings to anticipate the behavior of decision-makers in complex circurstances, is primarily a tool for facilitating understanding rather than for achieving specific prediction. A game proceeds too slowly to explore more than a few of the many alternatives that life could produce. Among the advantages of computer simulation for analytic purposes is that of speed in exploring the implications of various alternatives. With the speed of conventional computation multiplied by a factor of 10,000, operations that in the past seemed utterly inconceivable now become quite practical.

The Nemesis of applied social scientists up to now has been the hideous complexity of the problems with which they work. To predict by mathematical equations the exact location at which a bouncing ball would end up is child's play compared to the prediction of where a vacillating human being might decide to go. The natural sciences have succeeded in producing formalizations which yield specific predictions because they did not have to deal with the monstrous number of variables, non-linear and discontinuous ones at that, with which the social scientist must cope. It is a cliché that for this reason the practice of the natural sciences is easy compared to the practice of the social sciences. If the cliché sounds paradoxical, it is only because the social sciences are so fantastically hard that frustrated human beings give up trying to make them rigorous and fall back on punditry and intuition. The social sciences are easier than the natural sciences in the same sense that it is easier to play on a roulette wheel than to play a game of chess. To really predict by intellectual analysis where a roulette ball will end up would require such an effort of research and analysis that no one would think of trying it. The solving of a chess

move on the other hand is a manageable problem at which the player makes an effort. It is only for that reason that one has the illusion that a chess problem is harder than a roulette problem.

The evaluation of social factors by policy planners has had the character of spinning a roulette wheel. In almost any discussion of strategy or military policy one is likely to find at some point the statement that in addition to the considerations which have been analyzed there are also psychological, social, and political imponderables which are essentially unpredictable. But such human factors are imponderable only in the same sense as is a roulette wheel. They are just as lawful but just as hard to analyze. True, they can be analyzed in principle. Just as a physicist can state the laws of roulette wheel operation without knowing which number will come out, so social scientists know principles of human behavior without necessarily being able to predict. But for planners, the goal is to move as much of the calculation as possible from the realm of roulette, i.e., chance, to the realm of chess, i.e., strategy.

Now, for the first time in the history of most social sciences, some problems which have been grappled with only in abstraction begin to seem manageable for planning through computer operations. It becomes possible to reproduce systems complicated enough to represent effectively what happens in the real world.

This may be done either by pure computer simulation or by a mixture of computer and human simulation. Introducing human judgment at crucial decision points in a simulation has the advantage that one does not have to have a theory of how such decisions will be made, a theory expressed in a computer program. Intuition is allowed some play. At the same time in mixed simulations, as developed, first at RAND by Chapman, Kennedy and others (4), and later at the Systems Development Corporation for example, the computer controls realistic displays of the environment that are offered to the players. The actions of the players suggest what would happen in a real world and thus the necessity for a theory of human behavior is postponed. Pure computer simulations on the other hand have both the advantage and the disadvantage of making more of a demand for the elucidation of theory. The impressive thing is that in a number of instances simulation programs have been able to meet that challenge. A number of extremely complex kinds of human behavior have been successfully simulated by computer programs.

One dramatic example is the Newell, Shaw, and Simon (10 and 11) simulation of human theorem proving in geometry and other fields. Other examples are the simulations by Holland (6) and by Orcutt, Greenberger, and others (12) of the processes of a total economy. An example in the field of public opinion change is Robert Abelson's (1) simulation of campaigns for fluoridation referenda. Civil defense behavior and international relations have also been the subject of computer simulations. The radically augmented arithmetic capability available to social scientists enables them increasingly to meet the challenge to become policy advisers about the consequences of specific combinations of actions.

STRATEGY: AN ILLUSTRATION OF THE OPPORTUNITIES
FOR SOCIAL SCIENCE

The classical art and science of strategy is hardly recognizable in to-day's study of the subject. The practitioners have changed. What used to be the domain of the officer has become increasingly a subject matter for economists, mathematicians, operations researchers, and political scientists as well. The central subject matter has changed from wartime maneuver to peacetime deployment, for, as Davison notes, victories which used to be won after war began now depend critically on the forces in being at the moment of crisis.

Strategy in its broadest sense is the allocation of resources and the deployment of them to effect desired goals. But within this broad concept which applies equally to business, education, politics, and games, as well as war, military strategy concerns use of particular resources, namely armed forces, toward particular goals, namely the foreign policy objectives of nations. The revolution in weapons and the revolution in the interrelations between states has brought with it a revolution in strategy.

The characteristics of the revolution in strategic thinking may be seen in four changes in the past decade. These are: (a) the recognition of the importance of survivability and hardening of strategic weapons; (b) the recognition of the importance of maintenance of conventional strength in the nuclear era; (c) the recognition of the usefulness of separating weapon sites from civilian complexes; and (d) the recognition of the possibility and uses of a second strike counterforce strategy. Each of these major changes stems in considerable part from the work of civilian scientific researchers, including social scientists, mathematicians, operations researchers, and others. For example, the recognition of the importance of survivability and hardening was signaled and in a substantial part produced by Albert Wohlstetter's classic article on "The Delicate Balance of Terror" (16). That essay made the obvious, once recognized, point that the important measure of our strength was not the weapons on hand before an enemy strike, but the weapons and forces on hand and able to retaliate once such an attack had come. Stated in this form it is a banality which, like so many banalities, is easy to neglect until it has been pointed out. Stated in this qualitative form alone it also could not have produced a revolution in America's strategic posture. That required detailed, precise, quantitative analysis.

It took calculations, based on the Soviet economy, on intelligence information, and on estimates of human performance capabilities, to compute probable numbers of delivery vehicles which each side might have available at different dates, their range, circular error, and destructive capabilities. It took calculations of the population distribution, its mobility, and its housing and occupational patterns to estimate probable deaths. From all of this vast amount of complicated calculation came persuasive estimates showing that investment in weapon survivability rather than use of the same money for more bombs and missiles was eminently worthwhile. The intellectual analysis has profoundly affected the character of defense expenditures, the policy regarding overseas bases and alliances, and, in short, a large part of the whole American posture in the world today.

To what extent was this social science? That is perhaps an unimportant question. Is city planning social science? Is industrial planning social science? The minute one begins to cope with applied planning for a large system, every science has its contribution to make. The decision to harden strategic weapons is probably as little sensitive to behavioral information as any military decision one can think of. Yet economic calculations were clearly important. Also important was knowledge about population distribution and movements, as well as knowledge about possible human responses to crises and evacuation, public attitudes toward first and second strike policies, etc. All of these human factors, whether dismissed as imponderables and therefore entered into the calculations only by unconscious assumption or whether entered into the calculations seriously, are clearly elements of the decision.

We are making no attempt here to carve out a stake for social science. The right to participate in decision-making is not the prerogative of any dsicipline. It can be claimed only by making contributions. Recent contributions to strategy have been made by men from many fields. Kahn and Read are physicists; Bethwell, a mathematician; Wohlstetter, a philosopher; Kaufmann, Kissinger, and Brodie, political scientists; and Ellsberg, Hoag, Morgenstern, Rowan, and Schelling, economists. None of these men was doing in strategic analysis what he had been professionally trained to do.

The theory of strategy has developed as a combination of many disciplines. We have discussed its relationship to game theory, economics, statistical decision theory, operations research, and system analysis. It is partly the theory of bargaining. It is partly straightforward arithmetic of weaponry. What it now lacks more than anything else is the effective incorporation of political and social insights. It has often been assumed, for example, in strategic analysis that a posture once taken is known to, and recognized by, the other side. That is seldom the case. Communication channels are noisy and full of bias. It has also been assumed in most strategic analysis that each side had well-established preferences and that the problem was to so manipulate circumstances as to make the other side's ordering over the available alternatives correspond to one's own. Yet in point of fact a large part of the game consists of mutual influence on preference orderings. The Russians decide about much of what they want by imitating us. As Schelling points out, such attitudinal influences have not yet been scientifically taken into account

Nevertheless, even while using greatly oversimplified models, strategic analysis has done many useful things. Analysis of targeting systems has led us to understand now in a way that we did not before which domestic objects might be prime enemy targets. It is clearly advantageous to get such objects away from those other objects which we most desire to defend, i.e., our cities.* Another useful exercise of strategic analysis is the demonstration by some prominent strategic analysts that our own security depends upon directing fire at those weapons which might destroy us, i. e., that a second strike counterforce strategy is effective and feasible. Such analysis has been important in changing American posture from one in which the only feasible reaction to enemy attack was a paroxysm of

---

*See Schelling (14). Cf. also Robert McNamara, speech of June 16, 1962, in Ann Arbor, Michigan (Vital Speeches, 28 (1962), 626-629).

emotional retaliation against civil populations, designed to deter but which might serve us no purpose whatever if war ever happened. The same sort of analysis has demonstrated that massive retaliation is not the best response to all threats which we might face (cf. Knorr and Read, 8), and has restored careful consideration of the role and use of limited war forces.

WHAT CAN BE DONE TO PRODUCE A USABLE SOCIAL SCIENCE

If what has been said above is sound analysis of the present state of knowledge then the defense establishment is in a position to stimulate a significant burst of social science creativity which could prove most useful to it. To do so, however, it must equip itself to utilize and understand social science methods and advice. It must provide social science training for a substantial number of its officers. It should include more fundamental social science in the curricula of its military academies. The social science that is useful to future commanders is not applied courses on leadership, but courses that will enable the officer to maintain some discourse with the social scientist just as his physics course enables him to maintain some discourse with a physicist. To better use social science, the Defense Department also needs to maintain and strengthen social science in its in-house laboratories to provide transmission belts between the outside scientists and the military planners.

Also important, though perhaps not the most important point, is the role of the Defense Department as a major supplier of funds, for research. The plea for long-run and continuous funding of basic research has been made so often that it seems hardly necessary to raise it again, but it is easy even for the most research conscious policy-maker to forget how much he is drawing from the capital of the past. The crises of the moment—at the point of writing in June 1962, Berlin, Cuba, Vietnam, Laos— always seem so pressing as to make it hardly seem worthwhile to fight for support for researchers to work in a leisurely theoretical way. That seems to be a luxury, but it is not. Tomorrow will be on us before we know it, and if researchers have not been working on tomorrow's problems we will be much worse off. It is easy to underestimate our intellectual debt to the past. As Schramm and Knorr point out below, the influence of social science on defense thinking up to now has been diffuse, but that does not mean it has not existed. The appreciation of a concept such as that of cultural differences somehow enters the thinking of a man in his school years. From then on he is a more sophisticated observer of those around him, but he may not himself know the source of his ideas. Even much more specific ideas lose the labels of their origins. Insights, be it the spherical character of the earth or the sleeper effect of communication (which happens to be the technical social science name of the phenomenon we are now discussing), are hard to come by but, once accepted, become part of common sense thinking.

The current defense program draws heavily on the intellectual capital of the recent past. It has put into effect policies that were being advocated though perhaps not acted upon before the incumbency of the present administration. Its relationship to the Gaither Committee and the intellectuals associated with that, its relationship to the strategic thinking in RAND over the past few years, and its use of men who had the leisure and "luxury" of

an academic or basic research environment in those years are almost too obvious to need comment. Can we now identify men working on similar problems in similar positions of independence from the current decision process: men who five to ten years hence will be playing the role of the "Kennedy intellectuals" today? It is not clear that we are replenishing our capital.

Responsibility does not rest entirely upon the Department of Defense for creating an environment in which social science can contribute to the solution of its problems. There are things that it must do, but there are also things social scientists can do to change their ways toward greater usefulness.

Max Weber in his essay on politics as a vocation made a fundamental and profound distinction between two kinds of ethics—the absolute ethic and the ethic of responsibility. The follower of the absolute ethic asked only what the rule was and acted accordingly. The politician who is a disciple of the ethic of responsibility asks what the consequence is. We live today in a world where force is more dangerous than it ever has been. The absolute ethic condemns it. Unfortunately the condemnation of it does not abolish it. Thus one can adhere to the absolute ethic only by disregarding consequences which may be more violent than ever. The social scientist who would be responsible must recognize in military problems some of the most important issues, perhaps even the most important issues of our time. With a painful awareness of consequences, he must address himself to these problems. The social scientist who faces the problems with sobriety and who does not indulge himself in the luxury of mere condemnation and pontification will soon find that he is called on to calculate the answers to concrete policy decisions. Technology now exists to enable him to take a stab at so doing. Increasingly social scientists must abandon the ivory tower which enables them to design models that do not need to face the test of action and must strive to contribute to engineering calculations which can help achieve security in a world of risks.

REFERENCES

1. Abelson, Robert P., and Bernstein, Alex. "A Computer Simulation Model of Community Referendum Controversies," Public Opinion Quarterly, 27 (1963), 93-122.

2. Blackett, P. M. S. "Steps Toward Disarmament," Scientific American, 206 (1962), 45-53.

3. Bray, Charles W. "Toward a Technology of Human Behavior for Defense Use," American Psychologist, 17 (1962), 527-541.

4. Chapman, R. L., Kennedy, J. L., Newell, A., and Biel, W. C. "The System Research Laboratory's Air Defense Experiments." Management Science, 5 (1959), 250-269.

5. Hitch, Charles J., and McKean, Roland N. The Economics of Defense in the Nuclear Age. Cambridge, Mass.: Harvard University Press, 1960.

REFERENCES (Continued)

6. Holland, Edward P. "Simulation of an Economy with Development and
    Trade Problems," American Economic Review, 52 (1962), 408-430.

7. Janowitz, Morris. The Professional Soldier: A Social and Political
    Portrait. Glencoe, Ill.: The Free Press, 1960.

8. Knorr, Klaus, and Read, Thornton (eds.). Limited Strategic War.
    New York: Frederick A. Praeger, 1962.

9. Miller, Neal Elgar, et al. "Strengthening the Behavioral Sciences,"
    Science, 136 (1962), 233-241.

10. Newell, A., and Simon, H. A. 'The Simulation of Human Thought," in
    Current Trends in Psychological Theory. Pittsburgh, Pa.: University
    of Pittsburgh Press, 1961.

11. Newell, A., et al. "Report on a General Problem Solving Program,"
    in Information Processing (Proceedings of the International Confer-
    ence on Information Processing). Paris: UNESCO, 1960.

12. Orcutt, Guy H., et al. Microanalysis of Socioeconomic Systems. New
    York: Harper and Bros., 1961.

13. Rabi, I. I. Compton Lectures, 1962. Cambridge, Mass.: Massachu-
    setts Institute of Technology.

14. Schelling, Thomas C. "Dispersal, Deterrence, and Damage," Opera-
    tions Research, 9 (1961), 363-370.

15. Von Neumann, John, and Morgenstern, Oskar. Theory of Games and
    Economic Behavior. (2d ed.) Princeton, N. J.: Princeton University
    Press, 1947.

16. Wohlstetter, Albert. "The Delicate Balance of Terror," Foreign
    Affairs, 37 (1959), 211-234.

17. Zilsel, Edgar. "The Sociological Roots of Science," American Journal
    of Sociology, 47 (1942), 544-562.

II

ALLIANCES

W. Phillips Davison

The term "alliance" covers many different forms and degrees of cooperation among nations. Alliances may be close or distant; they may provide for joint military planning and action, or they may be restricted to more limited cooperation; they may be used as camouflage to hide the control of a weak nation by a strong one or may be entered into by two or more powers pursuing a common goal. A nation that is a member of several alliances ordinarily has slightly different objectives and expectations with regard to each. And within any one alliance, each partner is likely to have differing reasons for participation. One reason the United States supports the Inter-American alliance is because it provides an instrument that can be used against interference from outside the hemisphere; however, the alliance is supported by many Latin American nations in part because they believe it protects them against various forms of intervention by the United States—from within the hemisphere (Dreier, 6, pages 20-21).

The importance of distinguishing clearly among various kinds of alliances is underlined by communist efforts to present the Warsaw Pact as if it were the equivalent of the North Atlantic Treaty Organization (NATO). These efforts have been partially successful, and one not infrequently sees the Warsaw Pact referred to by some such expression as "NATO's communist counterpart." When viewed casually, the two structures do indeed have marked similarities. Both have a unified military command, with the top post going to the strongest coalition partner. In both cases the military command is given policy direction by a high-level civilian group composed of representatives of the member states, whose decisions must be unanimous.

When the Warsaw Pact is examined more closely, however, it becomes clear that the power relationships within this alliance differ radically from those within NATO. The unified command of the Warsaw Pact nations is headed by the First Deputy Minister of Defense of the Soviet Union, and his deputies are the ministers of defense or commanders-in-chief of the member states. Thus, the Soviet Ministry of Defense has a direct line of authority extending into the armed forces of all Warsaw Pact

W. Phillips Davison, PhD, is a Senior Research Fellow at the Council on Foreign Relations, New York. From 1948 to 1961 he was a staff member of the Social Science Department of the RAND Corporation. Davison has served as editor of the Public Opinion Quarterly and consultant to the Operations Research Office and the Department of Defense. He is author of The Berlin Blockade—A Study in Cold War Politics (Princeton University Press, 1958) and co-editor (with Hans Speier) of West German Leadership and Foreign Policy (Row-Peterson, 1957).

nations. The Political Consultative Committee can issue instructions to the Joint Command, it is true, but since these instructions must be unanimous the Soviet member of the committee has an effective veto over any with which he does not agree. In the absence of instructions the commander acts as he is ordered by <u>his</u> superior—i.e., the Soviet Minister of Defense. The Warsaw Pact thus appears to be principally a device to camouflage Soviet control of the armed forces of the satellite nations.*

The NATO command, on the other hand, is heavily dependent on the concurrence and cooperation of member states. If they do not agree on basic policy instructions to the commander, his hands are tied. Furthermore, each nation is free to decide what forces it will place at the disposal of the alliance.

A number of general trends can be observed in the development of alliances during the twentieth century. First, in view of changes in military technology, alliances have come to emphasize collective preparedness and strength in being rather than willingness to consult or to provide mutual assistance after hostilities have started. Second, a related tendency is for alliances to be concerned increasingly with measures short of military force. Traditionally, allies pledged to furnish each other with armed assistance or to observe benevolent neutrality; now they are likely, in addition, to consider the use of economic, diplomatic, and other instruments for defensive purposes Third, alliances in this century usually emphasize joint action in other fields besides defense and may make provision for increased economic cooperation, cultural interchange, and so on. Fourth, multilateralism has become more popular than bilateralism; while important bilateral alliances still exist in the free world, those that play the largest part in current thinking are all multilateral. Fifth, alliances seem to show an ever-increasing degree of organization. In the 19th century, pacts usually provided for consultation through diplomatic channels; more recent alliance systems have tended to provide for permanent secretariats, joint planning boards, and committees dealing with numerous specialized subjects.**

The first "modern" alliance, which exemplified most of these trends, was the Little Entente, sponsored by France before World War II. While it had no defined central military headquarters, as does NATO, it built up specialized staffs in several member countries, and engaged in a substantial amount of joint military planning. The North Atlantic Treaty Organization (NATO), the Southeast Asian Treaty Organization (SEATO), the Central Treaty Organization (CENTO), and the Organization of American States (OAS), the four principal alliance structures in which the United States is involved, have all developed even more extensive formal organizations. While these organizational structures provide for military planning, and in the case of NATO for unified command, they also have politic economic, and cultural functions. A simplified comparison of the functional organization of these alliances is shown in the chart below. The observations in the following pages apply mainly to these multilateral,

---

*The writer is indebted to Dr. Kazimierz Grzybowski for bringing the internal structure of the Warsaw Pact to his attention.
**This summary of alliance trends follows rather freely the analysis presented by Haugse (10) in a doctoral dissertation.

SIMPLIFIED COMPARISON OF THE FUNCTIONAL ORGANIZATION
OF MAJOR ALLIANCES WITH WHICH THE UNITED STATES IS ASSOCIATED*

(The civilian secretariat, headed by a Secretary-General, has
been omitted from the chart because it is common to all the alliances.)

	Political Direction	Military Planning	Command Structure	Economic Agencies	Cultural Activities	Other
NATO	NATO Council, and Council Deputies (Ambassadorial rank)	Standing Group, Military Committee, and Operational Hdqrs.	SHAPE & other Hdqrs. Various joint training institutions	None, but pressure for more economic consultation among members	Fellowships and visiting professor programs	Scientific co-operation; Information Program
SEATO	SEATO Council, and Council Representatives (Ambassadorial rank)	Military Advisers, and Military Planning Office	None (but some coordinated training and maneuvers)	Economic Committee, and Economic Services, Office of Secretariat	Cultural Committee, and Cultural Relations Office in Secretariat	Some research and anti-subversion activities; technical training
CENTO**	CENTO Ministerial Council, and Council Representatives (Ambassadorial rank)	Military Committee, and Permanent Military Deputies	None (but military planning staff has some training functions)	Economic Committee	Some cultural functions under Secretariat	Counter-subversion; technical training
OAS	Inter-American Conference and Council of the OAS	Inter-American Defense Board	None (Defense Board makes recommendations only)	Inter-American Economic and Social Council	Inter-American Cultural Council	Many specialized conferences and agencies: health, science, law, etc.

*Sources: 3, 13, 21, 25, 27.
**The United States, of course, is not a member of CENTO but is associated with it through membership in its Military Committee.

free world combinations, rather than to bilateral organizations or to pacts within the communist world.

WHAT CAN APPLIED RESEARCH CONTRIBUTE?

The aim of applied social research on alliances is twofold: to assist those who have to make political decisions about alliances, both low-level and high-level, and to help those who are concerned with the operation of alliance machinery. Mere statement of these goals does no , however, provide very much guidance to the researcher. Alliances are so central to current international political processes, they are so closely related to the domestic life of nations, and the combined military commands and central secretariats that are the visible portion of alliances have become such involved structures, that almost any increment of knowledge in the social sciences is relevant in some way to inter-allied relations and operations. It is difficult to say where "alliance research" begins and ends. Under these circumstances, one can only seek to identify those aspects of alliances that are most vital to the policy-maker and operator and then to specify research problems the solution of which would help to illuminate these aspects. Inevitably, some important subjects will be omitted and some blind alleys included. Any agenda for the researcher on alliances should be subject to frequent correction and revision.

Two forms of research are not considered here since they are regarded as belonging to the administrative rather than the scientific order. One form is the staff study, in which various alternatives are weighed on the basis of currently available information and a course of action recommended for the decision of the executive or commander. Also excluded is the process of securing material that is needed to implement or support a course of action that has already been decided on. Most research departments in industry and many research offices in government are concerned with activities of this latter type. The kind of research that will be discussed in the following pages tends to look critically at policies and to search for new forms of behavior. This type of research seeks to assist administrative processes and policy formulation, but is not a part of either.

EXISTING SOCIAL RESEARCH ON ALLIANCES

A spot check of research leaves one with the impression that relatively little recent work has been done on questions applying to alliances in general. Theoretical studies and empirical efforts applicable to more than a specific situation are equally rare. Apart from works on international law, the Library of Congress catalog shows fewer than a dozen volumes whose titles indicate that they are concerned with alliances in a generic sense. One important recent book in this category is a symposium, Alliance Policy in the Cold War, edited by Arnold Wolfers (32).* Also of

---

*Since this was written, an excellent general treatment of political factors in alliances by Liska (19) has appeared. The author considers such questions as reasons for alignment and dealignment, cohesion of alliances, negotiations among allies, and neutralism. Liska's book appears to be the most ambitious generic discussion of modern alliances that has appeared to date.

interest are a number of semi-historical studies, such as the Haugse (10)
dissertation on alliances in international relations that was cited above,
and a handful of publications dealing with economic aspects of alliances,
of which Hitch and McKean's (11) The Economics of Defense in the Nuclear
Age is perhaps the outstanding example. Journal articles that would fall
into this category of general studies appear to be equally rare.

Works on international relations offer another source for thinking
about alliances. Although most of these are confined largely to historical
summaries and to descriptions of current alliances, there are some nota-
ble exceptions. Morton Kaplan's (16) volume on international systems, for
instance, contains a partial theory of alliances. Starting from the changed
international balance of power system that emerged from World War II,
one can specify why alliance partners are no longer readily interchangea-
ble, as they were in an earlier era, and why nations instead tend to form
blocs based largely on similarities in political and moral values. A very
different, but no less stimulating, approach is that of Karl W. Deutsch and
his collaborators (4), who, on the basis of historical studies of political
integration. achieve insight into some of the major factors holding alli-
ances together. Nevertheless, even if one includes thinking about alliances
that has appeared as a part of broader works on international relations,
the volume of theoretical and general material is still very small.

By way of contrast, the quantity of research dealing with specific as-
pects of specific alliances is enormous. While large in amount, however,
research in this category is very unevenly distributed among different
alliances and subjects. NATO receives far more attention than all the
other alliances in which the United States is involved put together. For
example, the inventory of foreign area studies compiled by the State De-
partment's Division of External Research in April and October 1960 lists
approximately six titles dealing with NATO for every one dealing with
CENTO or SEATO. This distribution of attention is, to some extent,
guided by current opinions about the relative significance of the various
alliances. There is no good reason why the research effort should be
divided evenly among all of them but the fact remains that research tends
to be most lacking in those areas where ignorance is greatest and where
illumination may be most needed.

Within the existing body of studies on alliances in which the United
States is involved, three large groups can be distinguished. One group,
with which we are not directly concerned here, is made up of legal studies
that focus on aspects of international law, the interpretation of treaties,
and so on. A second group includes policy and strategy studies, mostly on
a rather general level. The titles of a few of the most important recent
books falling in this group reflect this level of generality: NATO and the
Future of Europe (Moore, 20); NATO and American Security (Knorr, 17);
NATO in the 1960's (Buchan, 1); and NATO: The Entangling Alliance
(Osgood, 22).* Journal articles falling in this group also tend to paint with
broad brush-strokes, although a few are more focused: e.g., Hoag (12) and
Wohlstetter (31).

---

*As an indication of the degree of importance currently attached to
this subject by American scholars, additional major politico-military
studies of NATO are now in preparation, and several are expected to ap-
pear in book form in 1963 or 1964.

Finally, there is a large group of historical and morphological treatments of alliances, (for example 28, 29). These usually describe how an alliance came into being and how its machinery works. Many of them are unpublished.

Most of the studies falling in this last group are more descriptive than analytical in nature and focus on the structure rather than on the dynamics of the treaty organization concerned. There are, however, a few exceptions (for example, 15, 18). A provocative title of a recent Ph.D. dissertation is Dow's (5) Benelux: An Experiment in the Compatibility of Sovereignty With International Cooperation.

Research studies focused primarily on related subjects frequently contribute heavily to understanding of alliances and their functioning. This category is, however, so large and diffuse that one can only give examples of kinds of questions treated. Among the general subjects of interest in this connection are neutrality, neutralism, collective security, international communication, international organization, and military strategy. Also relevant are economic, political, cultural, and opinion studies of nations that are members of an alliance, nations that are hostile to an alliance, and important neutrals. Th.s, of course, includes nearly everybody. A substantial amount of research has been done on all these subjects, but since it has ordinarily not been done with the problems of alliances in mind, it rarely is directly applicable to them. Such research has to be both adapted and supplemented if it is to contribute to alliance policy or functioning.

On the basis of this partial survey of relevant literature, two further impressions about the nature of existing research on alliances might be mentioned. First, relatively little use has been made of the techniques usually associated with sociology, social psychology, and anthropology. Most of the writing reflects the preoccupations and techniques of the students of history, politics, and military science. The second impression is that the existing research does more to uncover and focus attention on problems than it does to suggest policies or procedures. Thus it could be regarded as preliminary to more applied studies.

It is interesting to note in this connection that, with the possible exception of some specialists in international law, no group of experts on alliances has made its appearance in the United States or elsewhere. The political or military policy-maker looking for advice on this subject will usually have to turn to a generalist in foreign affairs or to an area specialist.

AREAS FOR ADDITIONAL RESEARCH

In view of the constantly expanding objectives and machinery of alliances with which the United States is associated, and the limitations of existing knowledge about them, further research in all the categories that have been mentioned above would be useful. The problem is one of deciding on priorities: Where should one start ' The following suggestions do not attempt to dea. directly with priorities, but include a number of questions that are probably amenable to social research techniques and to which even partial answers would be likely to produce results of direct or indirect practical utility.

Alliance Policy and the Nature of Alliances

The concept of an alliance among sovereign states is something that
has been inherited from ancient times; nevertheless, the properties of this
form of association have never been fully explored. In any case, the shape
and substance of alliances have been constantly changing. It is not always
clear whether given objectives can be achieved best through an alliance or
through some other form of cooperation. The possible uses of various
kinds of alliance, in both peace and war, have never been inventoried. In
some cases the United States has believed that it can achieve its ends
better by formally staying out of an alliance (for example, CENTO) than by
joining it. There is also the problem of withdrawing from alliances grace-
fully. Once a nation is a member of a pact it is often extreme.y difficult
to withdraw even though the alliance may have outlived its purpose.*

Questions about alliance policy and the nature of alliances that might
usefully be explored by social scientists are many in number. At what
point does an alliance become a confederation? What are the relative ad-
vantages of bilateral as opposed to multilateral agreements? What are the
advantages and disadvantages of using the alliance mechanism for economic
and cultural as well as military purposes? Is it possible to devise an
adequate typology of alliances? Are there other forms that can achieve
the same cooperation equally well or even better? Is international coop-
eration more firmly based on the pursuit by several nations of a common
interest in a body of shared values or on a bargaining situation where each
obta :.s a price for cooperation from one of the others?

A fascinating field for inquiry would be the unintended consequences of
alliances. Some students have suggested, for instance, that the political
and economic ties usually accompanying modern alliances can set in mo-
tion a chain of events that leads to results that were unanticipated by the
alliance-makers.

The complex organizational structures necessitated by alliances may
also have led to unanticipated consequences. A senior diplomat** has
noted that in his opinion training international staffs is one of the most
important functions of all alliances. If the various NATO staffs were not
in existence, he thought it would take the United States and its allies at
least six months after the outbreak of war to duplicate the level of ability
to work together that had been achieved through staff cooperation. Other
observers have mentioned that the experience of working in multi-national
headquarters has greatly broadened the horizons of the present generation
of military officers--both Americans and other nationals. The cumulative
effect of inter-allied operations may ultimately exert a powerful effect on
the picture that the alliance partners have of one another.

---

*Cf. Burnet (2). The author argues that England should not regard
each alliance in which it engages as permanently desirable.

**Here, and in the following section, the writer has drawn heavily on
discussions with officials who have had extensive diplomatic or military
experience in working with alliances. The judgments expressed are, of
course, the responsibility of the writer alone.

Several of the social sciences may be able to contribute to alliance theory by drawing on the body of thought already available in their own disciplines. From a sociological point of view, an alliance involves a pattern of relationships among groups. This is a branch of sociology that is relatively undeveloped. Many patterns of relationships among individuals are fairly well understood, but the factors involved in the functioning of a body that is made up of groups rather than individuals have received less attention. Nevertheless, industrial sociology, the sociology of race relations, and possibly other branches may provide insights that can contribute to basic thinking about alliance policy. Recent work on coalition formation, although it has been concerned mainly with small groups, may prove to have important application in international politics.* From an anthropological point of view, an alliance could be considered an example of cross-cultural communication and organization, and experience from previous anthropological research on these subjects should be tapped. Several of the social sciences can contribute valuable data about decision-making, both on the part of the individual and of the large organization. Further historical research as well as intensified political analysis of contemporary experience, could also yield a great deal more insight into basic questions about the utility and functioning of alliances than has been the case hitherto.

Specific Aspects of Alliance

Research that is directed toward specific aspects of alliance activities will in the long run also contribute toward building a body of generalizations about alliances. In the short run, it is likely to be able to achieve rather striking results in a number of cases, and it is here that the initial pay-off from research will probably be greatest. The paragraphs which follow give examples of the kinds of questions that could be explored:

The functioning of allied headquarters and planning staffs. Except for a few studies applying almost exclusively to NATO, very little is known about the way that allied headquarters and planning staffs actually work. The little that is known points immediately to a number of contradictions and problems. On the one hand, it is not uncommon to find a very strong allied esprit developing. Lord Ismay (14, pages 463-464), in his memoirs, vividly describes the emotional wrench that he experienced when he parted from his international staff at NATO. Similar observations have been made by lower level officers at international headquarters, and studies conducted during the Korean war indicated that this international esprit often extended to NCO's and privates.

On the other hand, stories of friction and near-friction at allied headquarters, while they are less likely to find their way into print, can be heard from almost any personnel who have served in the central staffs of any one of the alliances in which the United States is involved. It is

---

*Cf. Gamson (7). Gamson makes use of mathematical game theory to define conditions under which coalitions will form and to specify the pay-off that is expected by each participant. While the application of such models to real international situations would unquestionably be difficult, systematic attempts to do so might very well suggest policy alternatives that would not otherwise be available for consideration.

frequently reported that important papers are passed from one American to another at such headquarters, on the grounds that staff work is speeded up if allied officers can be by-passed. Sometimes this short-circuiting process is based on language, and in such cases British and Americans often work together in by-passing those who do not have English as a native language. Non-English speaking nations have sometimes sought to gain a larger voice in international headquarters by appointing personnel who were already proficient in English, but this often meant that the men concerned were less competent in the technical fields in which they had to work since their selection was based primarily on linguistic skill. In other cases, allied officers have been excluded from taking part in staff work because it was believed that they would not exercise sufficient care in guarding classified information.

It is also commonly observed that national identifications tend to make the work of international staffs more difficult, quite aside from language or security problems. Sometimes an inordinate number of relatively minor questions are passed along to the highest level for decision because personnel at lower levels fear that if they made the decision it would look as though they were trying to gain advantage for their nation. Alternatively, decisions are not made at lower levels because personnel feel that any mistake will reflect unfavorably on their country. Officials at an international training institution reported that they had found it necessary to stop war games before one side or the other won a decisive advantage because, otherwise, the national pride of those officers on the losing side would be injured.

The complexity of personal relations on an international staff is suggested by the story told by an American officer who served several years in an allied sub-headquarters. One of the other Americans on the staff was clearly a misfit and performed poorly, yet his superior, who was a Greek, always gave him a high efficiency rating. When asked why he did this, the Greek colonel replied substantially as follows:

"If I gave him a low rating the only result would be that he would hate me and all other Greeks for the rest of his life. Furthermore, the rating would be disregarded by you Americans because it was given by a foreigner who did not know the ways of your military establishment and therefore could not be trusted to evaluate the performance of an American. Therefore, it is best if I give him high ratings until he is reassigned."

Personnel who have served in international headquarters can usually tell stories of misunderstandings, unintended insults, and awkward social situations involving the nationals of two or more nations. Yet the same personnel ordinarily refer also to the warm feelings engendered by working together with allied nationals on common tasks.*

---

*Some of the apparently contradictory statements one hears with regard to the functioning of various allied headquarters may be explained by the fact that personnel have a high regard for the aim of the headquarters—that it function on a truly international basis—but are conscious that the goal has not been completely achieved. They can therefore be both enthusiastic and disillusioned at the same time. The goal of the SHAPE staff, as described by Goodpaster (9), page 258, for example, was to function as a unified organization in which all officers, regardless of nationality, worked for a common mission. This goal had apparently been achieved only in part.

It is probable that systematic studies of the functioning of allied head-
quarters and staffs could contribute substantially to efficient operations,
both by enabling us to learn more about the ways that an international
esprit is formed and by identifying friction points and suggesting ways
they might be done away with. It should also be possible to improve com-
munication among various national elements, to find ways of dealing with
national pride that would make it less of an inhibiting influence, and to
suggest forms of social relationships that would be most appropriate for
various allied staff headquarters.

This is one of the areas which is most amenable to social research,
and a relatively small investment should pay excellent dividends. Not only
would it be possible for research to assist the functioning of the head-
quarters or staff that was being studied, but the knowledge gained would
certainly benefit numerous other inter-allied activities.

Some questions that could not be answered by research might still be
formulated more clearly with the aid of systematic studies. For instance,
one student of the SHAPE headquarters has noted that, with 12 countries
that should be represented in important positions, key assignments are
proliferated.* Jurisdiction therefore tends to be divided among a number
of multi-national committees, and the decision process is made more dif-
f....t. This problem has already been identified by many of those working
at SHAPE, but by exploring it more fully, and by providing a judgment of
its impact on the work of the headquarters, research might be able to pave
the way for remedial political action that could be taken with minimum
offense to national sensibilities.

International training and maneuvers. Nearly all presentday alliances
involve joint international maneuvers, sometimes on a very large scale.
Most of them also entail some formal or informal military training on an
international basis. There has been some experimentation with the forma-
tion of units composed of personnel from different nations. For example,
a multinational support battalion for NATO's central army group, consist-
ing of one company each of United States, French, and West German sol-
diers was formed in 1960.**

At the present time, most international training activities are on a
one-way basis: that is, the United States trains personnel from allied
countries. But there is also a gradually increasing flow in the other di-
rection, and several truly allied military educational institutions are being
developed, such as the NATO Defense College in Paris.

International training and maneuvers offer a large number of areas
where social research could fruitfully be undertaken. One such area is
with regard to the use of training and maneuvers to increase the commit-
ment of allied personnel to the common cause. Since men of different na-
tions who work together on a project often develop a new sense of solidar-
ity with each other and with the various nations represented, a study of the

---

*There are 15 nations belonging to NATO, but Iceland has only rudi-
mentary armed forces, and Portugal and Luxembourg ordinarily do not
seek key assignments for their nationals.
**From New York Times, January 8, 1961.

conditions under which this mutual solidarity tends to form would make it possible for alliances to design new opportunities for joint efforts. It would also help to identify both the problems and potentialities of multi-national military units, including such specialized groups as missile-launching and submarine crews. Similarly, research could pretest training materials and procedures designed to increase allied solidarity.

While joint training and maneuvers may have a strong effect on the attitudes of the men engaged in them, they also can affect third parties. Situations used in training can demonstrate allied solidarity to potential enemies and to others. They may have a powerful impact on public opinion in allied countries. Similarly, the attitudes that allied nationals take back with them from military training in the United States, or from any allied training, can have a strong influence on public opinion in their respective countries. Joint German-American maneuvers in Europe, for instance, appear to have had a positive political effect in Germany as far as the NATO alliance is concerned. Some maneuvers and training devices could be designed specifically to achieve political as well as military effects. Here again, the chances of success would be greater if the range of political effects that might be achieved were studied carefully in advance.

The area of allied training and maneuvers is one in which there has been extraordinarily little systematic research to date, although there is a substantial fund of scattered experience on which to draw. Even a modest research investment in this field could be expected to pay big dividends.

The possibility of simplified languages. Communication both in allied staffs and among allied personnel in training, on maneuvers, and in multinational units might be substantially improved if increased attention were given to developing simplified languages. While progress is being made with intensive language study programs, there is scope for a great deal more imaginative thinking and experimentation. The concept of a "basic military English," for instance, has not been explored. Yet the need for such a simplified means of communication is indicated by the fact that simplified or adapted language forms have spontaneously sprung up at various places. The men of the new West German Air Force are accustomed to speaking of "gegrounded" and "offgetaked," and have adapted other English expressions to their technical needs. Commenting on this, a New York Times dispatch of August 9, 1961, observed: "Although Germanic scholars frown on the strange language, the knowledge of it has paid off for the pilots. Working within the framework of NATO, they never have problems when communicating with their allies."

A somewhat similar linguistic adaption occurred in the American Fifth Army during World War II. A language known as "Fifth Army Italian" gradually developed, and Americans found that they could learn it fairly well in several weeks. It involved many simplifications, such as all verbs being used in the infinitive, but it proved to be an adequate means of communication with Italian partisans and the Italian populace. In other words, in addition to the more systematic work that has been done on Basic English and other international languages, there is a substantial amount of experience that could be tapped in designing simplified means of communication for use in connection with allied operations.

The public relations of alliances.* Public images of alliances can be influenced through allied training and maneuvers, and less directly through the performance of international staffs. However, there are many other factors that enter into the public relations of each alliance. The strength of any international grouping of democracies is dependent in part on the degree to which it is supported by the public, and especially by certain segments of the public, in each member country. The success of an alliance in achieving its objectives may also depend on the extent to which it helps to deter potential enemies, the way it is regarded in third countries, and so on. Writing about the problem of maintaining confidence in NATO, Klaus Knorr (17, page 8) notes that "what people and governments think the facts to be is as important as the facts themselves."

One research approach that could be used in support of the alliances in which the United States is engaged, therefore, is that of exploring the ways the alliance, its activities, its weapons, and so on, are perceived by significant publics. Sporadic work along these lines has been undertaken by the U. S. Information Agency and by some private researchers, but a great deal more is required if the potentialities of alliance public relations are to be fully realized. This research should also study significant attitudes toward alliances with a view to discovering the structure of such attitudes and the way they are related to attitudes toward other political factors. In particular, it is desirable to know as much as possible about the meaning of alliances to different peoples and nations. For instance, for the United States an alliance is an honorable association and a symbol of strength. This image is probably related to the fact that Americans have been members of victorious coalitions in two wars in modern times. In those nations that have been on the losing side of coalition warfare, or that have been associated with alliances that were mainly instruments of domination, the prevailing image may be quite different.

It would also be useful to explore more fully the reasons that an increasing number of nations regard an "alliance-free" policy as a symbol of independence and to determine whether a form of association that would appeal to them could be devised. Specific questions for study are: What dangers and obligations is a particular alliance believed to bring with it? How much does it offer in the way of security and privileges? To what extent is an alliance seen as bringing advantages in the present and to what extent in the future?

Within alliances among democratic states it might be worthwhile to maintain continuous surveillance of opinion trends likely to affect the association. For instance, it would be technically possible to construct indexes of cohesiveness for an alliance and to use these as one way of judging the effort necessary to maintain an acceptable degree of ability to take collective action. Content analysis techniques might prove to be useful in gauging trends affecting alliances as reflected in representative publications, statements of political leaders, and so on. Opinion research and content analysis would make it possible to maintain a systematic check on the support given principal aspects of alliances. Such analysis would show, for instance, whether an alliance was increasingly regarded as an economic instrument, a military instrument, or a cultural bond.

---

*See also the discussion by Schramm, pages 46 to 74 below.

The way that various publics perceive an alliance can sometimes be influenced through publicity. Research can contribute a great deal toward planning and executing a public information program. Communications in support of alliances usually come through one of three channels: reports from commercial news media relating to activities of the alliance, speeches and statements by leaders in allied nations, and material issued by the alliance itself or by a nation or group supporting it. Opposing communications are also found in commercial news media or may be spread through other channels by hostile nations or groups.

To date very little work has been done on communications in support of alliances or on defense against hostile communications. Alliance public relations could be strengthened by studies of the impact on significant publics of major news reports, speeches by alliance leaders, and public information programs such as the one conducted by NATO itself and the various private groups supporting NATO.

Sometimes observers attach substantial significance to publicity and public relations activities. For instance, a New York Times report of April 17, 1961, attributed great importance to Vice President Johnson's speech at SHAPE headquarters on April 6, 1961, comparing it with the Zürich address of Winston Churchill in 1946 in which he called for a United States of Europe. Together with President Kennedy's defense budget statement and the appointment of Thomas Finletter as Ambassador to the NATO Council, this speech was credited with suddenly filling the whole alliance with an enthusiastic spirit. An earlier news report (in the New York Times of March 19, 1961) stated that NATO's Northern Command had revised its public briefings in an effort to make the deterrent effect of the alliance on the Soviet Union clear to Danes and Norwegians. Yet, in spite of the importance attached to public information, relatively little systematic work has been done to find out whether the impact of events, reports, and statements is indeed as great as sometimes supposed and to determine how the available instruments of public relations may be used most effectively in support of alliances.

Defensive research. Closely related to alliance public relations is the ever-present threat posed by hostile attempts to disrupt major United States alliances. These attempts usually consist of efforts to inflame existing irritations, to make use of threats and blackmail, or to foment subversion. Some studies of Soviet and other disruptive tactics have already been made. For example, Hans Speier (26, pages 95-110) presents a chapter on Soviet atomic blackmail. Additional studies of past and present attempts to disrupt alliances would be valuable as an aid to designing better countermeasures.

Examining Alternative Situations and Strategies

Somewhere between research concerned with alliances in a theoretical sense and research on questions affecting specific types of operations in particular alliances, there is an area of inquiry concerned with innovations and contingencies, e.g., future possibilities and alternative strategies. For instance, one would like to know as much as possible about the relationship between (a) alliance policies and (b) new weapons and strategies. What kinds of weapons and strategies are most useful in view of the requirements of a given alliance? Or, if certain weapons and strategies are taken as given, what impact are they likely to have on the alliance?

Similarly, it would be valuable to investigate the utilities of hypothetical alliances. Should efforts be made to extend the United States alliance system? Or would it be possible to encourage additional combinations of third powers in which the United States would not be a member? According to a newspaper report (30, page A-6) the Kennedy administration has been seeking to find a form of alliance that would appeal to the formally "uncommitted" nations. What would be the probable effects of forming "super-alliances," linking two or more of the present alliance systems with each other?

Various moves and policies that would tend to increase cohesion within an alliance, that would help to deter potential enemies, or that otherwise would help to achieve the aims of an alliance could be suggested and examined more systematically than has previously been the case. For example, some students believe that an alliance would benefit politically by having the armed forces of each major member scattered throughout the territory of the other major members, even though the military requirements for each deployment were not clear. Thus, contingents of British, French, and German forces could be stationed in the United States for training or other purposes at the same time that American troops were sent to Europe. In this way, it is argued, the people of the various NATO powers would become more truly aware of the interdependence of the Western nations. Others have asked whether allied maneuvers should not occasionally be held in or near the United States, as well as at distant points, whether the size of allied staffs should be increased in order to give larger numbers of personnel training in working together in international teams, and whether there would be advantages to having polyglot international military units in each major alliance.

Further questions have been asked about the executive machinery of various alliances. NATO and OAS grew up on a basis of past experience and with a historical background of cooperation among the nations involved. SEATO and CENTO have no such basis, but the tendency seems to have been to transfer to them the organizational structure that grew up in NATO. Yet there is no assurance that these organizational forms are indeed the most appropriate ones. It might pay to examine others.

Alliances have benefited relatively little from creative imagination, as far as their design and functioning are concerned. Padelford (23) suggests that NATO might be well advised to establish machinery somewhat like the European Consultative Assembly at Strasbourg and a recommendation to this effect was made by the NATO Parliamentarians Conference in November 1962. Dreier (6, page 170), has indicated that a sweeping strengthening and modernization of OAS is required if this alliance is to play its indicated role in the years to come. A number of unconventional ideas could be explored. For instance, could alliances among democracies be strengthened by asking each alliance member to elect observers to sit in the legislatures of other alliance members" These observers would serve to emphasize that alliances are between peoples as well as between governments.

It is also possible that a greater division of labor among allies should be sought. That is, each member of an alliance would be expected to specialize in those functions that it could perform best. While this is already the case in some instances, especially in the military field, the principle

could be carried much further. For example, some of the technically-
advanced allies of the United States might be able to do far more in train-
ing the forces of emerging nations than is now the case, and informational
and educational tasks in various parts of the world might increasingly be
divided up among allies, with each taking responsibility for those areas
and functions to which its capabilities are best adapted.

There are thus a large number of ideas for strengthening alliances
and assisting them in achieving their goals that require examination and
testing before policy decisions can be made on whether implementation
should be attempted. While responsible officials must make the decisions,
researchers can do a great deal to prepare the way for policy-makers and
enable them to consider many more possibilities than otherwise would be
the case.

Some research along these lines, particularly with respect to alter-
native strategies and organizational arrangements for NATO, has already
been undertaken, but a great deal more could be usefully done. The de-
velopment of political gaming techniques during the past few years offers
a promising way of studying the implications of innovations within alli-
ances.* For example, it would be feasible to arrange political games
focused on questions of particular interest to a given alliance and to use
as players informed nationals of the various nations involved. Such a pro-
cedure might eventually open new and unexpected avenues in inter-allied
relations.

Contributory Research

Any well conducted research program that contributes to knowledge
about international relations, or assists in the definition of foreign policy,
can be justified in part on the grounds that it will enable us to strengthen
the coalitions in which we are involved or to find ways to make them func-
tion more efficiently. Nevertheless, four broad categories may be men-
tioned as likely to contribute particularly to alliance functioning or policy,
although they may contribute to many other areas as well. Research in all
these categories is already available, but relatively little of it has been
undertaken with the problems of alliances in mind.

Research on foreign policy goals. Since alliances play a major part
in the pursuit of our foreign policy goals, it follows that the more precisely
these goals can be defined and the more we can learn about their relative
importance and about the degree to which each of them is espoused by
significant publics, the better we will be able to evaluate the effort that
should go into the alliances themselves. Clear definitions of goals also
are necessary for the benefit of the personnel who staff inter-allied head-
quarters, or who are otherwise concerned directly with the functioning of
an alliance. Observers have reported that men who feel they do not under-
stand what an alliance is supposed to achieve are usually poorly motivated.
Finally, the relative feasibility of specific goals is another important
variable. How much of our national effort we can and should put into the
support of alliances will depend in the last analysis on answers to questions

_____
*Goldhamer and Speier (8); and see McRae's discussion on pages 188
to 224 below.

pertaining to goals. Again, the caution is probably unnecessary that re-
search cannot formulate national policies. It can, however, assist the
policy-maker in discovering and defining national goals, and in examining
their implications.

Research on allies. When officials who have worked extensively with
alliances in which the United States is involved are asked what kinds of in-
formation would have been most useful to them in their work, they almost
invariably say that they would have liked to have had more information
about our allies.* This is especially true with regard to SEATO, CENTO,
and OAS, although some officials felt that they were inadequately informed
even in the case of NATO allies.

For alliances to function effectively, each nation requires compre-
hensive information about its partners. Information is especially needed
in five areas: national goals, power structure, significant political atti-
tudes, cultural characteristics, and the effects of the alliance on domestic
policy. What are the principal national goals of each alliance member?
How are these goals to be achieved? How are foreign policy decisions
made? Who has the power to commit the nation to a given course of ac-
tion? What are the most important political attitudes toward other na-
tions, current issues, and prominent personalities? What cultural and
social characteristics are likely to affect relations among personnel of
allied nations? In what ways does an alliance affect a country's domestic
policy? Will the alliance have an impact on the economy, is it likely to
bring about a shift in the power structure, and so on? These are the areas
of information that are frequently mentioned as being necessary or useful
for the official concerned with the functioning of an alliance.

A related question, frequently asked by those who deal with allies that
are in the stage of rapid political and economic change, concerns the di-
rection of development in these countries. The conduct of the alliance will
have a strong impact on the shape of certain military, political, and eco-
nomic institutions, and therefore it is important to know which lines of de-
velopment should be encouraged. Examination by political scientists and
economists of forms of organization that are best adapted to various
emerging countries would have a practical value for those concerned with
day-to-day decisions.

Cross-cultural communication. The study of cross-cultural commu-
nication can contribute to all aspects of international relations, but it is of
particular importance to keep communication channels to allies open and
functioning adequately. One sub-field of cross-cultural communication
that is particularly in need of additional exploration is how to negotiate
with friendly nations, both allies and non-allies. Substantial attention has
been given to negotiation with hostile states, and especially with the Soviet
Union, but it is often assumed that no problems are involved in negotiating
with friends. That this assumption is incorrect is attested by an experienced
foreign service officer in a rather whimsical article (24). He notes that
negotiating with friends is much more complicated and difficult than dealing

---

*One such official mentioned: "It was several months before I real-
ized that we weren't dealing with a nation in the western sense at all; we
were dealing with a group of powerful families."

with enemies—for one thing, one cannot ordinarily show mistrust, or re-
sort to calculated impoliteness. And possibly because they are in such
frequent contact, friends have a way of getting on each other's nerves.

Another aspect of cross-cultural communication that particularly
concerns alliances is that of propaganda and public information. This af-
fects not only relations among allies but also the relationship between the
alliance as such and other states—neutral and hostile. Advances in this
field will therefore assist in the functioning of alliances both directly and
indirectly.

The importance of person-to-person cross-cultural communication in
the context of alliances has already been mentioned. Any knowledge that
will help to smooth cooperation among persons of different nations will
assist in the operation of allied headquarters and international military
units and in the functioning of international activities of all kinds.

Knowledge about neutral and hostile states. Most of the categories of
information that are useful in the case of friendly states are required for
neutral and hostile states also, although the emphasis within these cate-
gories may be different. For example, the power structure of neutrals and
enemies is as interesting as that of allies, but it would be examined from
a different point of view. While aspects of allied power structures that
have a bearing on cooperation are particularly useful to know about in this
connection, in the case of hostile states it might be of relatively more im-
portance to know about strong and weak spots and in the case of neutral
states to be aware of the possibilities of change in one direction or the
other. Alliance policies are governed to a very large extent by what is
known about potential enemies and neutrals, and, therefore, research deal-
ing with their goals, capabilities, power structures, and significant politi-
cal attitudes is likely to be important for the operation of any alliance. In
general, knowledge appears to be somewhat better and more readily avail-
able to the policy-maker with respect to hostile states than with respect to
friendly or neutral states.

RESEARCH STRATEGY: IS IT POLITE TO STUDY FRIENDS?

Research on political matters is a neighbor of intelligence. Perhaps
this is the reason why a reluctance to study political conditions in friendly
nations, or to be studied by friendly nations, can be detected. Attitudes
here are somewhat similar to attitudes governing propaganda: it is re-
garded as perfectly in order to propagandize a hostile state, but it is
thought to be of doubtful morality when propagandistic communications are
directed to friends. Another reason for reluctance to conduct certain types
of research in friendly nations may be that this kind of activity can be
taken as indicating distrust of the government of that nation. It is often
felt that the proper method of obtaining information from another govern-
ment is through established channels.

Whatever the reason in each case, many (although not all) research
activities among friendly states have been inhibited. This is particularly
true with regard to survey-type studies that involve collecting information
directly from individuals, as opposed to library research. Not many years
ago, a United States agency wished to conduct a survey in a friendly country

but, after much hesitation, decided that it would be desirable to check with the foreign office of that country before proceeding. Accordingly, an American made an appointment with the appropriate official and explained the problem. After he had listened a moment, a broad smile spread over the foreign diplomat's face, and he revealed that his country had been contemplating a very similar survey in the United States but had feared that this might arouse American official resentment.

As this incident suggests, there is often something faintly ridiculous about the inhibitions applying to research among allies. Furthermore, the United States in particular, and to an only slightly lesser degree, Great Britain, France, and West Germany publish about themselves large amounts of information in categories that are useful in connection with the functioning of alliances. It is not, therefore, as though any of these nations were seeking about others information that they are not willing to disclose about themselves. Nevertheless, the existence of suspicion about some kinds of research, especially among emerging nations without fully democratic governments, should be recognized and faced as one of the problems besetting researchers in this field.

Two measures for dealing with this suspicion are suggested for further exploration. One would involve having alliances themselves sponsor research. That is, along with other functional offices in the secretariat of each major alliance, there should be an internationally-staffed research office. Its researchers should be given a charter for studying questions relating to the functioning of the alliance. Some precedents for such an activity already exist. SEATO, for example, has a research unit in Bangkok (25). NATO, CENTO, and the OAS sponsor certain research activities. All of this research is limited in scope, and in the case of SEATO it is focused mainly on the communist threat to the alliance. Nevertheless, research offers a base on which to build, and it is probable that alliance-sponsored research would not only overcome a large part of the resistance to studies in friendly countries but would also make it possible to benefit from better cooperation on the part of significant groups in the country being studied.

A related suggestion is that greater use be made of cooperation between private American social science research organizations and research groups in friendly countries. Since World War II a number of such groups have been formed in all parts of the world, and their numbers and capabilities are steadily increasing. Many of them, both within and without the universities, have traditions of cooperating with American research institutions and have been willing to cooperate in sponsoring and executing studies of mutual interest. Indeed, the best research strategy for the study of problems relating to the functioning of alliances would in itself constitute an interesting subject for inquiry.

REFERENCES

1. Buchan, Alastair. NATO in the 1960's. New York: Frederick A. Praeger, 1960.

2. Burnet, Alastair. "Too Many Allies?" Political Quarterly, 32 (1961), 146-156.

3. Central Treaty Organization. The CENTO Council Meets in Tehran. April 28-30, 1960. Ankara: CENTO, 1960.

4. Deutsch, Karl W., et al. Political Community and the North Atlantic Area. Princeton, N.J.: Princeton University Press, 1957.

5. Dow, James E. Benelux: An Experiment in the Compatibility of Sovereignty With International Cooperation. Unpublished Ph.D. dissertation, University of Minnesota, 1959.

6. Dreier, John C. The Organization of American States and the Hemisphere Crisis. New York: Harper and Row, 1962.

7. Gamson, William A. "A Theory of Coalition Formation," American Sociological Review, 26 (1961), 373-382.

8. Goldhamer, Herbert, and Speier Hans. "Some Observations on Political Gaming," World Politics, 1. (1959), 71-83.

9. Goodpaster, Andrew J. "The Development of SHAPE: 1950-1953," International Organization, 9 (1955), 257-262.

10. Haugse, Eugene S. Alliances in International Relations Since 1920. Unpublished Ph.D. dissertation, University of Nebraska, 1954.

11. Hitch, Charles J., and McKean, Roland N. The Economics of Defense in the Nuclear Age. Cambridge, Mass.: Harvard University Press, 1960.

12. Hoag, Malcolm W. "What Interdependence for NATO?" World Politics, 12 (1960), 369-390.

13. Inter-American Defense Board. An Introduction to Mutual Security Planning by the American Republics. Washington, D.C.: The Inter-American Defense Board, not dated.

14. Ismay, H. L. I. The Memoirs of General Lord Ismay. New York: Viking Press, 1960.

15. Jordan, Robert S. A Study of the Role of the International Staff/Secretariat of the North Atlantic Treaty Organization During the Tenure of Lord Ismay as Secretary General. Unpublished Ph.D. dissertation, Princeton University, 1960.

16. Kaplan, Morton A. System and Process in International Politics. New York: John Wiley & Sons, 1957.

REFERENCES (Continued)

17. Knorr, Klaus (ed.). NATO and American Security. Princeton, N.J.: Princeton University Press, 1959.

18. Lawson, Ruth C. "Concerting Policies in the North Atlantic Community," International Organization, 12 (Spring 1958), 163-179.

19. Liska, George. Nations in Alliance. Baltimore, Md.: Johns Hopkins University Press, 1962.

20. Moore, Ben T. NATO and the Future of Europe. New York: Harper & Bros., 1958.

21. North Atlantic Treaty Organization. Facts About the North Atlantic Treaty Organization. Paris: NATO, 1962.

22. Osgood, Robert E. NATO: The Entangling Alliance. Chicago: University of Chicago Press, 1962.

23. Padelford, Norman J. "Political Cooperation in the North Atlantic Community," International Organization, 9 (1955), 353-365.

24. Ramsey, James A. "Negotiating With Friends," Foreign Service Journal, 36 (1959).

25. Southeast Asia Treaty Organization. Collective Security, Shield of Freedom. Bangkok: SEATO, 1959.

26. Speier, Hans. German Rearmament and Atomic War. Evaston, Ill.: Row, Peterson & Co., 1957.

27. State Department, U. S. Organization of American States. U.S. Department of State publication no. 6625. Washington, D.C.: Government Printing Office, April 1958.

28. Thomas, Jack E. SACEUR and SHAPE: A Study of Peacetime Preparation for Coalition Defense, 1950-1956. Unpublished Ph.D. dissertation. University of California (Berkeley), 1959.

29. Van Alstyne, Richard W. "The Shape of NATO," Current History, 39 (1960), 129-132.

30. Voss, Earl H. "U.S. Seeking Anti-Red Alliance With Neutrals," Washington Star, July 11, 1961, A-6.

31. Wohlstetter, Albert. "Nuclear Sharing: NATO and the N+1 Country." Foreign Affairs, 39 (1961). 355-387.

32. Wolfers, Arnold (ed.). Alliance Policy in the Cold War. Baltimore, Md.: Johns Hopkins University Press, 1959.

III

IMPACT OF MILITARY POSTURES IN PEACETIME

Wilbur Schramm

Since the beginning of history, nations have used their military forces, between wars, as instruments of national policy. In ancient Athens, Sparta, and Rome, as in modern Switzerland and Israel, the armies have provided technical education and taught the civic virtues to young men (81). For the Roman princes and the Arab caliphs, as for the new and developing countries of today, the armies have provided police power. In modern states like England, France, and Germany, military forces have been used in peacetime as instruments of foreign policy, to threaten enemies and misbehavers, encourage allies, and support negotiations. For the Soviet Union and the United States, military postures have come to be important moves in the Cold War.

The significance of these latter developments is that military forces are now being used in peacetime to communicate something to other countries. Part of what a nation communicates by means of its military postures in peacetime is controlled and purposeful; part is incidental to the existence and activities of a large organization. In recent decades the purposeful use has increased in proportion. But purposeful or not, the military activities of leading nations in the Cold War still communicate widely. The United States military establishment--with its great size, its enormous budget, the pervasiveness of its installations, its close relationship to industry and to the economy, and the frightening destructiveness of the weapons it controls but hopes never to have to use—is a subject of deep concern both to our friends and to our enemies. To many, including neutrals, it sometimes seems like a bull in a china shop; and even the most friendly bull is likely to cause even the most tolerant proprietor of a china shop some moments of anxiety. In a more serious sense, it stands as chief barrier between the impatient communist movement and the Western countries. It is not surprising, therefore, that the military postures and plans of the United States communicate messages of deep interest to many countries, whether or not we intend them to do so.

That being the case, it is manifestly better to have those postures communicate what we want them to, than something else. It is better to consider the psychological implications of our military posture before we

Wilbur Schramm, PhD, is Professor of International Communication and Director of the Institute for Communication Research at Stanford University. Dr. Schramm has done research on four continents. He has represented various United States and United Nations organizations at a number of international conferences, has served on the Defense Science Board and other government panels, and is a Consultant to State, USIA, Health, Education and Welfare, and other federal departments, and also to the Ford Foundation and other organizations.

adopt the posture, than to evaluate those implications afterward. Inasmuch as we are going to have to disclose military developments, plans, and actions, in any case, it is well to consider how most effectively to disclose them.

THE NATURE OF THE PROBLEM

The problems surrounding our military posture are more difficult and pressing than they might ordinarily be, because of the situation in which they are set. The present is a situation not unlike the one Hobbes described three centuries ago (quoted by Singer, 101, page 90), and about it we might make the same comment he made:

> [nations] " ... in the state and posture of gladiators; having their weapons pointing and their eyes fixed on one another; that is, their Forts, Garrisons, and Guns upon the Frontiers of their Kingdomes, and continuall Spyes upon their neighbors; which is a posture of War."

It is in this very tense situation, akin to war, that military developments, movements, and postures are communicating essential information, which is the more valued and the more attended to for the reason that it is "hard" fact rather than "soft" words.

As Brodie (9) noted in his thoughtful paper on military disclosure, when military demonstrations are used as instruments of foreign policy, they can be used to convey either purpose or capability. It is obvious that they can convey either friendliness or unfriendliness. For an example of the former, consider the use of military means for a mercy airlift; for a small but mighty example of the latter, take the occasional case of military personnel conducting themselves objectionably in a foreign country. Using military postures to convey purpose or intent, of course, may involve a certain element of risk. In a tense state of international affairs it is difficult to divorce capability entirely from intent, or either from attitude and feeling tone.

Contrasts in Use of Military Postures

Let us remind ourselves how the two great powers in the Cold War have been using military postures short of war. One of the favorite ways to reveal capabilities, in recent years, has been the disclosure of a major power-related scientific development. Perhaps the most notable of these was the first Soviet atomic explosion which had the distinction of being announced to the world by the President of the United States. Equally impressive was Sputnik I, which ascended unannounced and then, evening after evening, hung in the sky for hundreds of millions to see. On each May Day the new Soviet airplanes have been flown over the parades and the watching air attaches. And the young pilot Gagarin captured the imagination of the whole world by making the first flight in space.

Soviet tactics have been remarkably uniform. The Soviets have announced significant developments which were prepared in secrecy and revealed only when successful. They have gained through surprise. Developments have been displayed at a time, in a way, and with a fanfare to make the most of their impact. Our tactics have necessarily been different

from these because of the importance we place on an open society. Except for the original bomb developments, our new developments have tended to come in the full glare of publicity, and we even staged a bomb "spectacular" at Bikini and minor atomic fireworks under public view in Nevada. We have made known our failures along with our successes and by pre-announcement have minimized the possibility of surprise. Thus, our rockets are readied, fueled, and fired in a grandstand atmosphere. Our new airplanes are debated at length in Congress before they are built. Our capabilities with unusual weapons are discussed and sometimes excoriated in public. Our first space rider takes his flight on all the television networks. Our industrial contributions to defense soon reach the magazines; our atomic submarines soon reach the toy counters.

There is no suggestion here that the advantage in all cases is with the nation able to maintain secrecy about its new developments as long as it wishes. As a matter of fact, an open society provides a great advantage in credibility. Shepard's flight, although less an accomplishment than Gagarin's, may actually have had a comparable impact because it was announced in advance and shown in detail as it happened. The point to note about these tactics is not that one is necessarily better than the other, but that they are different and require different rules and methods. And they probably have different effects.

So much for the tactics used to communicate capability. There is less difference in the tactics used by the United States and the Soviet Union to communicate purpose and intent. Both maintain garrisons as a symbol of their intent to maintain borders—the Soviet Union in Eastern Europe, the United States in numerous countries around the Soviet perimeter. Both have intervened with troops to support friendly regimes—the Soviets bloodily in Hungary, the United States bloodlessly in Lebanon. Khrushchev has threatened military intervention in Egypt, Cuba, and Berlin; the United States has sent a fleet to the China Sea, and kept the Strategic Air Command (SAC) in the air. Both sides have used airlifts: the Soviets in Laos; the United States to support the United Nations in the Congo, to carry medical aid and supplies to earthquake-striken Chile, and to supply the besieged people of West Berlin. Both nations have coupled their military actions with assertions of commitments to maintain peace, although the Soviets, with their long-continued peace campaign and their disarmament gambit. have been a little louder about it.

The chief difference has been in relation to _internal_ developments of countries. At the time of writing this study in 1961, the Soviets have not hesitated to support "good" revolutions or other social changes engineered by their partisans, openly in places like Laos or Cuba, secretly through their paramilitary wings in many other places. The United States, with no worldwide party, has nevertheless tried its hand at supporting revolutionary activities, successfully in Guatemala, unsuccessfully at the Bay of Pigs in Cuba.

The Soviet Union has been communicating through these postures and strategies a confidence in the ultimate and "inevitable" victory of a communist world order and a willingness to aid and defend all movements in that direction. Strategically, it is behaving like a boxer who keeps a big right hand cocked while he jabs away with his left. That is, the Soviet Union maintains a powerful central capability (a) to keep the communist

bloc intact, (b) to use, if an appropriate time comes, for the grand international blackmail, and (c) to fight an all-out war if necessary. Meanwhile, the Soviet Union chops away at the periphery of the non-communist world, using its party as an instrument, internal discontents and aspirations as movers, and revolutionary movements as chief actors. Thus it has gained ground in Southeast Asia and Cuba; and countries in Asia, the Middle East, Africa, and Latin America, tester on the brink. The delightful possibility must have occurred to the Soviet leaders that they can possibly win without ever using that big right hand. They may be able to chip away the strength of the non-communist bloc, bit by bit, until the structure falls.

Until 1961, when this study was written, the United States has communicated through its postures and strategies a resolve to deter and contain— to deter the Soviet Union from a nuclear attack and to contain the communist forces within their borders. At the same time, it communicates an intention to help the new and poor countries make a free choice politically and economically. It adopts essentially a defensive posture: it has a big right fist of its own but will only throw that punch if the other side throws its big punch first; it will use its lighter punches to defend noncommunist countries against aggression. Thus being on the defense, the United States yields the choice of battleground to its opponent. The Soviet Union declares the socialist countries a "peace zone," where no social or governmental change except planned official change is to be recognized, and the rest of the world a "war zone," where social and governmental change are things to be sought.

The disadvantages of the United States stance are evident. It is not surprising that we are communicating, by means of our military postures and actions in this period, a certain amount of ambiguity and uncer... inty. A large part of the military budget is going for deterrence, but just what is it we are prepared to deter? Is it only a nuclear attack on the United States, or a nuclear attack on one of our major allies, or something else? Are we prepared to use nuclear weapons in a smaller war? Given the well-known American belief that all-out nuclear war can hardly ever justify its costs, can we ever make the "big punch" credible? Or can the other side chip away at the walls of the fortress without fearing our right hand, at least until he drops his bombs on us?

These questions also arise with respect to the policy of containment. Where are we prepared to draw the line? Where do we use our troops? Where, if at all, and how, do we defend against the internal subversion and revolution which is the communist pattern? Soviet weapons and ammunition have been used in Southeast Asia, but chiefly the troops have been Southeast Asians. This is the pattern. It is entirely possible that the Soviet blueprint calls for no invasion at all by communist troops of one of these countries, or none until a very late stage in the scenario, long after most of these countries are supposed to have fallen to internal pressures aided by the paramilitary party members. Thus, if we defend only against invasion, we may never have a chance to strike with either the left or the right hand.

This is one implication of our military posture that has been of concern both to us and to our friends. Another is the fact that our opponents seem so free to exploit all the revolutionary movements and expressions

of mass discontent in the developing countries and the colonial states. We, on the other hand, have been cast in the role of defending the status quo. They have the dynamism; we have, or have had, the task of supporting a number of regimes which have little to recommend them except the ability to keep order.

Of course, the purpose we communicate with our military postures can hardly be more effective than the policy communicated. If the message we are communicating is ambiguous (perhaps deliberately so) or ill-conceived, then it is necessary, before correcting the message, to ask whether the policy should be clear and whether it is well conceived.

Scope of the Problem

The discussion above concerns a policy problem, a strategy problem, and a tactical problem. The first of these problems is obviously serious, but it is out of bounds for this study. The third, or tactical problem, is clearly within bounds; it is the problem of how most effectively to communicate, with and on the basis of our military postures, what we have to communicate. The second, the strategic problem, is partly within, and partly out of, bounds. It is out of the purview of this study insofar as it involves the interactions of strategy and policy. It is in bounds insofar as it involves the interactions of strategy and tactics.

I am going to suggest three large problem areas that invite special attention and lie within the parts of strategy and tactics which are properly of concern in this paper.

One of the three problem areas involves the possible unintended results of the military postures adopted by the United States and the Soviet Union. Clearly, the presence of so much destructive capability in the world at one time, the competitive nature of nuclear levels, and the amount of frustration and hostility being generated constitute a dangerous situation. The situation is a firecracker that might go off at any time for any one of a variety of causes: accident (e.g., a satellite off course or a misinterpreted blip on the radar), catalyzation (hostility between the major powers brought on by a third power), escalation (a progressive rise in the destructive power of weapons and forces used in a peripheral war), or spread (of a peripheral conflict into a central one) (67). In view of these contingencies, it is proper to inquire what tactics, and what military postures, will make an undesired explosion more likely or less likely. This problem area might be identified as the problem of international tensions related to military postures. (See 6, 8, 15, 16, 17, 18, 30, 53, 62, 67, 76, 90, 101, 102, 103.)

A second area might be called the decision-making problem. By controlling military postures and disclosures we are seeking to affect the decisions of certain other powers—enemies, allies, neutrals. What do we know about the way decisions are made by those powers? In particular, what do we know about their patterns of risk-taking, and their decision-making under stress and threat and in ambiguous situations? To what extent are such decisions likely to be rational rather than irrational? How do the decision-makers respond to threats? At what level of confidence can we predict how they will respond to a given military posture, communicated in a given way? Given the best knowledge of these matters we

can assemble and the best estimate of risk we can make, how can we maximize our strategic use of military postures? (See 31, 55, 56, 64, 72, 75, 79, 91, 92, 97, 104, 105, 107.)

The third area is the communication problem. This is the question of what and how we can most effectively communicate by means of our military postures, given a policy goal and a set of assumptions concerning the decision-makers we are seeking to influence. How can we best make our messages credible? How can we avoid misperception of our intentions? How can we decide between different methods of disclosure? How can we best deal with the problem of multiple audiences? For, as Milburn (70) remarks, the whole world listens in to the conversations of the United States with the Soviet Union. (See 21, 47, 58, 59, 63, 94.)

These are the three problems--rather, the three approaches to the same problem--which we intend to discuss in the following pages.

SOME CONTRIBUTIONS FROM BEHAVIORAL SCIENCE

Behavioral science has already contributed substantially to our understanding of the psychological impact of our weapon systems and our military postures. The examples we are about to give are all from the unclassified literature of scholarship. In addition to these we may assume that there has been some behavioral research in this field, done by or for the military establishment, and given a security classification. But no classified sources have been used for this study.

One of the areas to which behavioral science has contributed is our knowledge of the psychological effects of different weapon systems. The United States Strategic Bombing Surveys (111), conducted in Germany and in Japan after World War II, showed the great resilience of civilian life and work under such bombing as was possible at that time. These and related findings were analyzed by Iklé and others to try to determine the amount and kind of destruction necessary to disrupt, to a given extent, the industrial economy and production. The effect of atomic bombing on the two cities where it occurred has been many times analyzed, both in the spirit of science and in the spirit of history and drama. Michael (68) concludes that there is still insufficient evidence, either from United States or Japanese sources, to enable us to predict how long or how well a city population would stand up against atomic bombing.

It seems probable, however, that the idea of atomic bombing, especially after 16 years of conditioning, generates an emotion which is quite different from that aroused by the idea of ordinary bombing or ordinary warfare. As a matter of fact, great differences have been found in soldiers' reactions to different weapons even though one kills as dead as the others. For example, Finan (29) found that among American troops in North Africa, the German 38-millimeter gun was the most feared weapon, with the dive bomber next. With more combat experience, fear of artillery increased, and fear of bombing decreased. Goldhamer (39) found that napalm was the most feared weapon among Korean and Chinese prisoners of war in Korea, although when the prisoners at a later date were asked to rate weapons in terms of fear and effectiveness they rated artillery first, bombing and

napalm second. Goldhamer concluded that the psychological impact of a weapon is greater:

> when it is not possessed by the troops who are subject to attack by it;
> when it cannot be seen and no evasive action is possible;
> or when attack with the weapon can be long continued.

Certain additional hints as to the possible psychological impact of modern weapon systems are given in the series of studies of disaster, conducted under the auspices of the National Research Council* and by others. These analyses of the social disorganization that follows great explosions, floods, windstorms, and other natural disasters offer some base from which tentatively to extrapolate probable patterns of behavior in case of nuclear attack. Pepitone and others (78) have explored psychological reactions to a hypothetical attack by poison gas. Reviewing all the evidence, Kahn (52-54) and Bleicken (7) have made impressive estimates of the social effects of nuclear warfare.

Another area in which substantial progress can be recorded is the very important one which represents the intersection of politics, diplomacy, and military strategy. The RAND Corporation has worked probably more than any other research group in this area. Examples of their useful work have been Speier's studies of German Rearmament and Atomic War (108) and of Soviet Atomic Blackmail (109) and Davison's study of The Berlin Blockade (22) which points out the profound effect of the peacetime use of a military weapon--the airlift.

In addition to the RAND work there have been important contributions from a number of university centers of political and international studies, among which must be mentioned those at Princeton, Harvard, Massachusetts Institute of Technology, Yale, Northwestern, and Stanford. In the last few years, some of the work at these centers has been stimulated, and a number of projects have been initiated by a group at the China Lake Naval Ordnance Training Station under Milburn. (See 11, 69-71, 80, and others.) The central problem studies by the China Lake group is deterrence. They have brought a number of civilian scholars, including psychologists, political scientists, mathematicians, and anthropologists, into the program and have interesting studies under way in the areas of decision-making, game theory, and historical and political analysis. Little of this work has yet been published.

Still a third area in which substantial progress may be noted--although great problems still remain unsolved--is the study of attitudes and values throughout the world. A large number of separate studies have been made of values, attitudes, symbols, and customs in different countries. The Human Relations Area Files** have contributed substantially to our store of information on these matters. These Files were one of the bases for a series of area handbooks,*** which assembled cultural information on

---

*Editor's note: See George W. Baker and Dwight W. Chapman (eds.), Man and Society in Disaster (New York: Basic Books, Inc., 1962).

**The Human Relations Area Files are maintained by Human Relations Area Files, Inc., Yale University, New Haven, Connecticut.

***Area handbooks are currently produced for military use by the Special Operations Research Office of American University, Washington, D.C.

different countries in systematic form. C. E. Osgood of the University of Illinois is now working on a cross-cultural study of semantic structures, using his semantic differential. Hadley Cantril and L. A. Free at the Institute for International Social Research, Princeton, New Jersey, have made a number of penetrating studies of symbols and opinion in other countries, and the U. S. Information Agency barometer studies have contributed to our understanding of climates and patterns of foreign opinion. There have been a large number of anthropological and sociological studies of political and cultural units throughout the world—such as Indian villages, Latin American haciendas, Pacific islands. And whereas we are very far from knowing all we need to know about the value and symbol systems of the cultures which we should study, still we certainly know a great deal more than we knew 20 years ago; and our knowledge of these subjects is now more firmly grounded in science and is less dependent on the lore and the memories of "old hands."

We have also built up a great deal more knowledge of the channels and processes by which ideas and values are communicated between and within cultures. There have been numerous studies of media and systems of communication and of the "flow of the news" between countries. Content studies of the foreign press and broadcasts have been done in great number. George (34) has made up an extremely helpful analysis of the degree to which it was possible in World War II to predict enemy policy on the basis of broadcasts. And finally, it should be noted that there have been a number of st dies of the relations between United States and foreign nationals at bases on foreign territory. Some of these have been published; some have not. They are obviously relevant to our understanding of the impact of military posture.

SOURCES OF ADDITIONAL CONTRIBUTIONS

The preceding examples are of studies which have been helpful and which are clearly related to the problems we are considering. Now the question is: Where can w. look in the psychological and social sciences for additional help?

It would be pleasant to be able to say that there is available a basic and general theory of international conflict, by means of which to illuminate the particular kind of conflict behavior which is our topic. There is, however, no such adequate theory available (see 92, 93). A great deal is known about the psychology of individual conflict, and something is known about the social psychology of intrapersonal and small-group conflict. Some of this promise to be useful to our purpose, but great caution must be exercised in extrapolating from it to the larger conflict patterns. International conflict is very unlikely to be describable solely in terms of "individual conflict writ large." Useful correctives are available from history and political science, for example, in such a notable work as Wright's (114) A Study of War.

It also would be pleasant to be able to say that we have available a general theory of decision-making, but this is not the case despite such work as Snyder's (64, 105) interesting postulates derived from history and

political science and the rapid development of game theory.* Game
theory, which originated in pure mathematics and was first used to illumi-
nate some economic questions, has made a profound impression on the
social sciences since it was introduced by Von Neumann and Morgenstern.
(112). There is no doubt that we have much more to learn than we have so
far learned from game theory about our particular problem, which is es-
sentially an immensely complicated n-person non-zero-sum game.

Yet a word of caution is needed here. The problem is to move from
the clear, rational world of mathematical games, where alternatives,
probabilities, and payoffs are all assumed to be known, to the not com-
pletely rational and largely unknown world of human conflict. Simon's (99)
critique of game theory as a practical tool is applicable here. "First," he
says, "we need a description that recognizes that alternatives are not given
but must be searched for.... Secondly, when a choice is made among al-
ternative policies, there is nothing simple or trivial about the task of de-
termining what consequences will follow on each of the alternatives....
Nor are comparisons among alternatives usually made in terms of a utility
function. We are more often concerned with finding a satisfactory alter-
native than with finding the best alternative.... In human problem solving
not only do we search for alternatives among which to choose, not only do
we search for the consequences that will follow on these alternatives, but
we search for the problems themselves." Therefore, the problem in using
game theory, immensely promising though it is, will be to re-insert some
of the human and complicating elements which were necessarily omitted
when the act of competition was simplified in order to permit the making
of a general theory with respect to it. Of this we shall have more to say.

In this connection, however, it should be mentioned that there have
been efforts to "game" international relations. Guetzkow (42) has de-
veloped and experimented with a mathematical simulation of inter-nation
behavior. RAND has played political games on the model of war games
(see 23, and other RAND studies). These activities are promising both for
training decision-makers and for exploring policy.

Although the application of game theory to national decision-making
and international relations is still in the realm of theory rather than
practice, there is one body of scholarship which is immediately useful.
This is the long accumulating series of studies on comparative govern-
ment, and especially the studies that deal with how different governments
make decisions. This literature is now of considerable size. In recent
years, under the leadership of Almond and others,** attention has turned
from the study of "ideal types" of government to the study of how govern-
ments actually work, how influence is exerted, how policy is formed, and
so forth. This makes the work much more useful in the study of such a
problem as we have made the subject of this paper.

---

*See 63, 73, 95, 96, ·· 112.

**Since this chapter was written, more of this material has become
available, following upon: Gabriel Almond and James Coleman (eds.), The
Politics of the Developing Areas (Princeton, N.J.: Princeton University
Press, 1960).

There is a great deal of research on the psychology of threat and ag-gression and the behavior of human beings under stress. Comparatively little of this research has so far been applied to the problems of national decision-making and international relations. (See 24, 26, 38, 61, 78, 87, 95, 97, 101, 115). The studies of aggression are especially apropos. They have examined a whole taxonomy of responses to threat produced in the laboratory or in the field, ranging from panic to complete rejection of the threat.

There is also a rich literature on interpersonal and intergroup per-ception. Anyone attempting to predict what a given military posture will communicate must take account of the effect of predispositions and group pressures on the interpretation of messages. Let us mention a few differ-ing examples. The "Mr. Biggott" studies (19), for example, show how completely a prejudiced person can miss the meaning of anti-prejudice propaganda and interpret it as support for his own position. A series of studies by Asch (3) and others show that group pressures can dramatically affect the way an individual perceives something; for example, an indi-vidual can see a line as substantially longer or shorter than it actually is, according to how the rest of the group report they see it. Another group of studies, by Festinger (28) and others, (e.g., Katz, 57) has shown the length to which individuals will go to avoid dissonance by seeking informa-tion which supports, rather than challenges, their beliefs and attitudes. For present purposes, the net effect of these many studies has been to emphasize the great importance of knowing as much as possible about the values, beliefs, goals, symbols, social positions, groups, and institutions of the people whose opinions we value with respect to our military postures.

There are also many studies on the communication process and the dynamics of persuasion and attitude change. These include the systematic experiments of Hovland and his colleagues (e.g., 47), the studies of per-sonal influence by Lazarsfeld and others (58), and the convergence of atti-tude and cognitive theory in the important 1960 summer issue of the Public Opinion Quarterly edited by Katz (57), and including work by Rosenberg, Osgood, McGuire, and others. All this material, which concerns the process by which a person arrives at an opinion and by which a communi-cation can change that opinion, is of obvious pertinence to our topic.

Finally, there are a number of other relevant studies in the fields of international relations, political history, and political sociology, the varied nature of which may be indicated by mentioning Dahl's (20) studies of power, the attempts to apply equilibrium models and "system" models to international relations (41, 99), Janowitz' (50) sociology of the military establishment, and Pool's (79) essay on "Public Opinion and the Control of Armaments."

This is a highly miscellaneous catalog, and purposely so, because the intention has been to illustrate the variety of scholarship that is potentially helpful in the problems under discussion in this study. For the most part, this scholarship has not been put together or interpreted so that it focuses on our problems. Some of it is not yet developed to the point of usefulness. But clearly there is more usable information than has been employed, and methods and tools have been demonstrated by which still more may be obtained.

THREE PROBLEMS

Let us now return to the three problems mentioned earlier.

The Problem of International Tensions
Related to Military Postures

The essence of the first problem is that military postures, capabilities, and threats are being used to maintain what Winston Churchill called a "delicate balance of terror" in a world full of hostility and fear. The question is how to keep the effects of those postures, communicated capabilities, and threats under control—how to use them to influence decision-making where we want them to be influential without setting off the enormously destructive forces not available to both sides in the Cold War.

One thing behavioral research tells us is that we must beware of treating the making of decisions as a purely rational matter. Fifty years of clinical psychology and psychi- ry have made startlingly clear the ex-.... .. -which human conduct is governed by emotions, by primitive aggressive and sexual drives, and by deep motivations of which the individual is himself often unaware (e.g., 30, 32, 49, 57, 115). Nor has the rather remarkable conclusion of Abel (1) in 1941—that in no case has the decision to use war "been precipitated by emotional tensions, sentimentality, or other irrational motivations"--stood up under later study. Zinnes and others (115) for example, found reason to suspect that the chief actors in 1914 disregarded what they knew of relative capabilities and probable outcomes under the emotional pressures of the last few weeks immediately preceding the outbreak of war. On the decisive day, the Kaiser felt trapped by circumstances, unwilling to go to war but unable to do otherwise. He felt that he was being forced into war by England and France; they felt they were being forced into war by Germany and Austria. The emotional situation had simply been allowed to get out of hand.

In particular, the psychological mechanism of projection is assumed to bulk large in international relations. Projection is a device by which we see someone else in our own image. For example, we assume that our enemy sees a problem as we do. When he takes action on the problem differently from what we would have taken, we consider that he is acting irrationally or capriciously. On his part, he assumes that we are acting hostilely or capriciously because, as he sees the problem, his action is the only right one. It is extremely hard for two angry individuals to realize that they may be seeing the same problem in different lights. (See 2, 30, 36-38, 76, 77, 101, 110.)

Gladstone (36) suggests that projection may be influencing the relations of the United States and the Soviet Union, each blaming the other for frightening action with neither realizing that its own actions frighten the other. Other scholars, citing their own experiences in the Soviet Union, argue rather persuasively that each nation is tending to see the other as a "mirror image": Ego assumes Alter to be using the same frame of reference, so that when Alter sees as straight what Ego sees as crooked, ther Ego assumes Alter to be wrong or malicious. Osgood (76) especially has written at length on this problem of projection in international relations and has emphasized the effect of tension and emotional drive on the tendency to stereotype all perceptions and decisions--that is, to render an individual

less able to see anyone else's point of view and to force him to operate with narrowed perspective and shortened foresight.

This stereotype tends to bring on what Merton and others have called "the self-fulfilling prophecy." Frank (30) has written on this topic out of his experience as a psychiatrist:

> "This is seen clearly in the stereotype of 'the enemy'.... The fact that the enemy—whoever he may be—is viewed as completely untrustworthy is a major source of tensions leading to war. The terrible thing about the mutual distrust of enemies is that it is justified. Some enemies are untrustworthy to begin with, but all become so eventually. Enemies cannot trust each other because each is forced to act in such a way as to justify the other's misgivings. This is an example of...the 'self-fulfilling prophecy'."

This psychological model for the escalation of misunderstanding and hostility into conflict is not unlike Richardson's (83-85) mathematical model of an arms race.

The implication of existing research and theory, therefore, is that there is a very real danger in a "deterrence" situation of projecting our own image on the enemy, of allowing our decisions and strategies to be stereotyped, and of being caught up in an irreversible process which will fulfill our own worst prophecies. (See 11; also 4, 8, 14, 17, 69-71, 90, 101-103.)

In practical terms, the research raises a question regarding the limits of utility of threats and negative rewards. We know something about reaction to threats. If threats are huge enough, they tend to be denied or rejected (47). On the other hand, in a tense situation, a threat that is not denied tends to call forth answering hostility.

But are we missing a possibility if we use our military postures only as threats to deter attack? As Milburn (71) points out, threats of punishment may suppress the threatened behavior but will do little to change underlying motives. Furthermore, outside threats, as we know from small-group research, will raise the cohesiveness of the group threatened. However, offering rewards may significantly change the direction of effort and motives and reduce the hard cohesiveness of group hostility. Military postures can be used to remove or protest against threats as well as to make threats. They can be used to reduce pressures as easily as to raise them. This is a point on which recent writers on deterrence have tended to converge. As Pool and Gleicher (80) ask: What can we support in Soviet policy? What can we favor, and reward? Milburn (71) asks what rewards we can offer the Soviets in return for what behavior on their part? One of the most powerful papers in this vein is by Osgood (76). He points out that the policy of mutual deterrence includes no provisions for its own resolution and that the most likely resolution is nuclear conflict—escalation from little wars to big ones, preventive war, pre-emptive war, or accidental war. He then proposes that we conduct the "arms race in reverse," by making small unilateral reductions of force or withdrawals from territory—always taking care that our heart-land or our second-strike capability is not affected—and accompanying such acts by explicit invitations to the other side to reciprocate. The problem, of course, is to find the sequence of

actions, the ways of announcing them, which will be most likely to elicit genuine reciprocation from the other side. Osgood believes it can be done and that by so doing we can reduce "world tensions to a level where serious negotiations can be entered into and significant agreements successfully concluded."

In any case, there is every reason to think that a use of military pos-ture to combine rewards and threats will be more effective than one that is made up of threats alone. The nature of the rewards to be offered is a matter, of course, that will require very serious study.*

The Problem of Influencing Decision-Making

The essence of our aim in influencing decision-making is, of course, to influence those nations whose decisions are important to our national welfare by means of our military postures in peacetime. Incidentally, we may make friends or enemies and influence public opinion, but, if we do, those results are incidental to our main purpose. Therefore, we must face up to a large number of questions about how decisions are made in general and in particular countries, and how they may be influenced.

Decision-making in one of the great powers today is an extremely complicated process. It is fully to be understood by no such simple ac-tivity as knowing the chief decision-maker or reading the history of pre-vious decisions, although each of these is helpful. Deutsch (25), in a 1957 article, asks a great number of questions about decision-making in a situ-ation of international tension that illustrate the complexities we are deal-ing with. Some of his questions are freely paraphrased here:

> Who are the political decision-makers? What are their backgrounds,
> interests, memories, levels of anxiety, images of their own roles, and
> of other pertinent roles? Who are the elite groups, the significant
> members of the mass, and what are their images of their roles in
> decision-making? What changes are occurring or about to occur in
> these lineups? How are political decisions made? What are the rela-
> tive effects of current information vs. past memories and stereotypes
> in the governing elite, the non-political elite, the active members of
> the non-elite, and the politically passive members of the non-elite?
> What is the stratification of influence and of opinion? How large or
> prominent is the share of the mass media and the share of word-of-
> mouth communication in influencing opinion? What are these shares
> in regard to current information and to accumulated past information
> recalled in the decision process? What is the effect of the non-elite,
> and the decision-makers' image of the non-elite's expectations, in the
> making of decisions? Within the mass communication sector, and
> within the word-of-mouth communication, what are the levels of atten-
> tion devoted to a given "enemy" country or people, or to a conflict
> issue? What is the ratio of favorable to unfavorable information in
> this news flow, and how specifically is attention focused on a conflict?
> What are the expectations of war or other violence? What part of this
> information is directly traceable to the government?

---

*Cf. Schelling's discussion below, pages 228ff of the promises implied by threats and their relation to military postures.

These, of course, include none of the important substantive questions such as those dealing with perceived national goals, national ideologies, world conditions and relationships, and the like, which obviously enter into decisions. It would be impossible to know such a process as decision-making in any foreign country (or, indeed, in our own country), in its full complexity. And yet we have a general understanding of the process, and the methods by which to fill in the outline specifically for any country, as well as a great deal of specific information on certain countries.

In a conflict situation, attitudes tend to harden and alternatives are restricted for the policy-maker. Dahl (20) writes, "The dilemma of the political leadership is this: Only if public opinion is fluid and undecided will the full range of theoretical alternatives be open; to the extent that public opinion hardens, alternatives are foreclosed." But effectiveness in foreign policy, as he later points out, depends finally upon the willingness of a nation to support its policy-makers when they are threatening as well as when they are conciliatory. Thus, internationally hostile attitudes in a population may generate more support and more tax money behind a competitive foreign policy but will also restrict the number of kinds of decisions that can be made. Dahl (20) says that what begins "as an expedient may end-in a trap, with all exits barred save those leading to catastrophe." Deutsch (25) has proposed that a certain level of hardening of attitudes might actually serve as an "early warning system" of a dangerous international situation. Be this as it may, any attempt to influence decision-making in a situation clearly must take into consideration the hardness of attitudes in a given country and the resulting freedom or non-freedom of decision open to the leaders. They cannot be expected to make a decision that is beyond their freedom to decide or that is too far from the opinions and attitudes around them; and it may be necessary to frame a policy to change opinions in the country before desired political decisions can be expected.

So far we have been talking about decision-making in national terms. Let us now shift the focus to the individual decision-maker. Here the research tells us chiefly that there are many ways to make decisions, many different ways of bargaining, many different patterns of choosing strategies, many different levels of risk taking. Therefore, the personal characteristics of any individual who has at least partial freedom for political decision-making become an important part of what we need to know if we are going to influence his decisions. What, for example, do we really know about Mr. Khrushchev's way of making decisions? What do we know about how he reacts to threats? What kind of news and information gets through to him? What are the chief influences on him? What is his image of the United States and its intentions?

We know from many research studies that there is a special danger in making decisions under threats or pressures. Here the alternatives tend to be foreshortened and the emotional element to be larger. Very often in foreign policy we are forced into the "quandary" type of decision. This is a decision in which an individual or an organization is pulled or pushed in two differing directions at the same time. The situation produces disorganization, as well as autistic hostility which tends to be taken out on other parties and often involves the decision-maker in a series of reciprocal acts of hostility. Theoretically, at least, it would be well to keep some of our decision-making capability, during a tense situation, relatively isolated

from day-to-day tensions and concentrated on longer plans, broader alternatives, and more imaginative strategies. The problem of how to do this has never been satisfactorily solved.

Studies of game theory and gaming are not often directly applicable to the act of political decision-making, but they throw a new light on some favorite political (and poker) axioms—for example, that bluff is a valuable strategy more often than not and that it is useful more often than not to avoid commitment early in the game (see 104). This is not exactly what game theory says. There are some conditions under which it is necessary to bluff and some when it is useful to withhold commitment. But on the other hand, there are even more situations when it is highly dangerous to bluff and when it is highly advantageous to commit oneself early. The study of these conditions and the attempt to translate them into military postures, movements, and strategies might prove to be a most rewarding activity.

But game theory becomes most interesting to us when personality elements are built into it. When this is done, then it becomes clear that in actual life personalities override the clear mathematics of the theory. For example, one study by Scodel, Ratoosh, and Mibas (95) demonstrated that the actual dollar value of expected payoff had very little relation to choice of strategies. That is to say, there seemed to be a value in victory, or even in competitiveness, for its own sake, quite apart from what was thought to be the main reward. Another piece of research by Willis and Joseph (113) included a great many pairs of subjects playing a two-person game in which only by cooperating could either win anything. When pairs were asked to cooperate, they did so; but when they were not asked to co-operate, it was only rarely that they did so. Their natural competitiveness kept them from trying the strategy of cooperation. A similar study by Scodel, Sayer, Ratoosh, and Lipetz (96) resulted in only two out of 41 pairs learning to cooperate. The others were either unable to view the game as anything except a competitive activity or were afraid of the possible ego-deflating experience of being double-crossed.

We do not need game theory to make us aware that the posture of international competition may be easier to achieve than the posture of international cooperation. But these experiments are highly challenging in that they raise the question of how individual determinants relate to general and universal determinants in the making of a policy decision. To put it another way, we know little about the extent to which the patterns of decision-making are culture-bound and personality-bound. For example, are the results of the game theory experiments we have just mentioned peculiar to the North American culture, or are they generally applicable? If we want to be able to predict a Soviet response to a particular sequence of military postures, to what degree would it be useful for us to understand the personality qualities of the Soviet leaders, to what degree the patterns and values of the Soviet government and state, and to what degree the general principles of competitive decision-making?

So far as the decision-making problem is concerned, then, much more work needs to be done before theory is of major assistance. Present scholarship warns of some of the dangers of decision-making under threats and pressures and about the personal differences in patterns of risk taking and strategy making. Scholarship also fascinates (and often frustrates) us

by the implications and possibilities of translating game theory from the clear, untroubled world of mathematics into the turgid world of individual and national behavior. But it is clear that the most immediately useful paths of scholarship are the politically sophisticated case studies of how relevant decisions are made, how information from military postures gets into the process of decision-making, and how information interacts with the other forces that appear to be potent in the situation.

The Problem of Communication and Persuasion

The problem of communication and persuasion is chiefly tactical. Essentially it is how to arrange and use our military postures, movements, and disclosures so as most effectively to communicate whatever we want to communicate in support of the strategy we have decided upon.

One thing available research tells us about this problem is the incompleteness and unsatisfactoriness of foreign news in every country of the world. The news flow is inadequate. What news is transmitted is passed b, very country through its own ideological filter. Some countries, including the Soviet Union, exercise a considerable control over news and use it for their own purposes.

Even if the flow were heavy and unrestricted, however, there would still be a problem of misperceiving the news. A reader can read and understand only in terms of the values he holds and the experiences he has stored away--his frame of reference. Two individuals can communicate successfully only to the extent that their frames of reference are relevant to each other (see 74 and 94). Thus completely successful and unimpeded communication is unlikely even between contiguous countries and friendly cultures. However, communication is easier in such cases than between countries whose cultures are widely different, where the flow of information is scant, and where the governments are not friendly.

The problem of being heard, then, is the first communication problem, and the problem of being understood is second.

Being heard is partly a matter of access to facilities and partly a matter of the kind of messages one sends, their timing, their spokesmen, and their competition for the audience. The research supports the ancient axiom that deeds speak louder than words, but deeds and words together speak louder yet. One Sputnik is worth many thousand lectures on Radio Moscow, and one Gagarin is worth an uncounted number of propaganda releases; but after Sputnik and Gagarin, people were more likely to notice the propaganda. The United States airlift to Berlin was worth millions of words of encouragement; and after the airlift, as Davison (22) has shown, the citizens of Berlin took a new interest in what the papers of the United States and Western Europe were saying about them.

Of course, it isn't every day that one can orbit a pilot in space or supply a besieged city by airplane. Therefore, the timing of a message becomes important. If it answers a current need, interest, or anxiety, it is more likely to get attention. If it is reported by a person who has visibility, and who is trusted, it is more likely to get attention. This is one reason to let respected foreign nationals see military demonstrations and report back to their own people.

Being understood is chiefly a matter of understanding the audience. We can ha..lly ever know too much about the value structures and the symbol systems of the people we talk to. In particular, we need to know (a) what our intended audiences know and think of us, (b) what they know and think of war and of nuclear and unconventional weapons, and (c) what their experiences have been with war. Whatever we communicate to these people can only be in terms of what they know and believe and value.

The United States has a cruel audience problem in that almost everything it communicates about military plans and postures is received by multiple audiences. Nearly everything said about military capabilities to Congress, for example, can be overheard by the American public and countless foreign listeners. Nearly everything said to foreign listeners can be overheard by the Congress and the American people. Since other audiences listen in, it is often hard to tailor a message to a given audience.

One difficulty in always having multiple audiences is that considerations which are necessarily complex tend to be dangerously simplified, and considerations that are necessarily simple tend to be dangerously complicated-- everything thus tending toward a middle ground. One example of this is the frequently heard and too simple idea of the relation of armament reduction to tension and tax reduction. Pool (79) has shown most convincingly that if we abolished nuclear warfare, we might find the bill larger, rather than smaller; and "far from necessarily entering into an era of good feeling, we might find ourselves having to fantastically increase our military effort so as to be able to deter Soviet invasion of, for example, Iran or Berlin with conventional weapons." It is not to say that there may not be good reasons to abolish nuclear arms, but these reasons should not be confused with economy or peace. Another facet of the two-audience problem is the fact that military disclosures and postures sometimes frighten our own people more than they frighten our opponents. For example, it is doubtful that Soviet or Chinese Communist leaders have been scared out of fighting by anything they have read about a potential United States capability for the use of chemical or biological weapons. But in our own country the thought of using such weapons has shocked and frightened many people.

To be heard, to be understood are important; the third step is to be believed. An important ingredient is credibility of spokesman. This has been the priceless asset of BBC for many years. It is this ingredient which makes national leaders, speaking officially, command special attention. Nobody can speak adequately for President Kennedy or Chairman Khruschchev. Thus, in visiting this country in 1959, Krushchev had an audience and attention that no other Russian could possibly have had; and President Kennedy's interview in Izvestia in 1962 put forth the American viewpoint in a way that no other American could. Spokesmen like these communicate with their deeds as well as their words. Think of the extraordinary effect it would have on attitudes toward civil defense if the President slept in a bomb shelter!

The unusual effect of the reporting to the world by the President of the United States of the first Soviet nuclear explosion calls attention to a body of research on overhearing. Festinger and others (unpublished research) have shown that when one is hidden and overhears a message, one is less likely to doubt that the message is bona fide. That is to say, one drops his defenses against manipulation. The implication is that there are often

gains to be made by not communicating directly to a given target. If the
target country thinks we are trying not to tell them about a military de-
velopment, it may get more attention. If the target country intercepts the
message, it may sometimes stand out more sharply than if communicated
directly.

To be heard, understood, and believed are necessary; but that is not
enough. We want also to make changes. We want to plant ideas and change
attitudes, and ultimately to influence the making of decisions. It is easy
to plant facts and ideas. Changing attitudes is more difficult, and there is
a large and complex literature on the subject, which we have no intention
of trying to summarize here. But scholars from several different strands
of psychological thought have agreed lately with remarkable unanimity that
one of the most powerful mechanisms for attitude change is the human be-
ing's train toward cognitive consistency (57). By this we mean the need
most persons feel to bring their beliefs and attitudes, and their attitudes
toward related objects, into line. Thus, Osgood (57) shows that if Alter,
whom Ego admires, espouses a cause against which Ego feels strongly,
then Ego will not be comfortable until he resolves that incongruity—most
likely by reducing his favorable opinion of Alter and raising his opinion of
the cause: Rosenberg (57) shows that if a person changes the affective
component of an attitude, then he will be driven by the strain toward con-
sistency to bring his beliefs into agreement with the new affect. For ex-
ample, if Ego comes to believe that a person or a policy or state of affairs
furthers some end he values, he will tend also to "like" the person or policy
or state of affairs. If Ego is persuaded to engage in a considerable amount
of role playing in which he expresses arguments contrary to his belief, he
will tend to reorganize his beliefs so as to bring them more into line with
his public expression. These findings are consistent with the "strain
toward balance" demonstrated by Newcomb (57) and the strain to get rid
of "cognitive dissonance" demonstrated by Festinger (28, 57). The impli-
cation of these findings is that one need not necessarily make a large and
dramatic change in order to change an attitude, but rather that some
change can be accomplished by implanting even a small discrepant fact or
belief, or encouraging even a small amount of discrepant behavior, because
the effort to bring the cognitive patterns into consistency with the discrep-
ant element will result in some general reorganization.

In this problem area, then, we are in somewhat better shape than in
the previous two. Although much research needs to be done, and many
facts (for example about audience knowledge and attitudes) need to be filled
in, still if the question of policy and strategy can be satisfactorily answered,
then there is a great body of knowledge on which to base communication
tactics.

In the tactical area, of course, we can call, not only on research, but
also on news and public relations experience, and on the kind of hard- and
clear-headed analysis of the problem illustrated by Brodie's (9, pages 299-
301) paper relating to military demonstration and disclosure of new weap-
ons. Here is some of Brodie's counsel on how to communicate military
capabilities:

"A weapon which, once known to the opponent, could be countered by
him in a relatively short time should as a rule not be disclosed for
demonstration purposes, however great may be its immediate tactical
efficacy. Conversely, a weapon which dramatically increases capabili-
ties and which is not easily countered (e.g., atomic bomb, long-range
rocket, radioactive gases) lends itself readily to demonstration uses.

"The most important secret about many weapons or systems is simply
the fact that they exist. Once it is ascertained that that fact is known
to the opponent, there is little likelihood that he will be assisted ap-
preciably by further disclosure which falls short of providing him with
details of performance and the manufacturing process. ... Where the
disclosure of existence has already occurred or where it is more or
less inevitable, a wide area is opened up in which further disclosure
for demonstration value may be exploited without incurring serious
penalties from such disclosure.

"The demonstration value of disclosure is enhanced, and the penalties
reduced, as the phase during which disclosure occurs moves from
prototype through production model through possession in substantial
numbers.

"The demonstration value of a particular weapon is likely to depend
far more on how it fits into the over-all strategic situation than on its
individual tactical efficacy. Thus, for the United States vis-á-vis the
Soviet Union, an A-bomb carrying bomber of very high speed and, at
the same time, substantial range will have much greater demonstration
value than an atomic-powered submarine. Similarly, a weapon which
greatly aids the defensive in land operations (e.g., radioactive gases,
tank destroyers) will, under the circumstances likely to obtain for
some time, be of much greater demonstration value than a weapon
which aids the land offensive.

.   .   .   .

"Disclosure should never be inadvertent (as frequently happens), but
always considered, deliberate, and for a purpose. The prescription
is thus not for less care in security, but for more flexibility of policy
on the part of the security guardians.

.   .   .   .

"Disclosures will obviously be more effective for demonstration pur-
poses during a peri of tension than during one of relative tranquillity.
But the method of disclosure, once it is decided upon, is also impor-
tant. Disclosure may be made to appear an inadvertent result of war
games, or a concomitant of conventional military demonstrations, or
it may be "leaked," or it may take place with considerable fanfare and
histrionic effects (e.g., Bikini). Each method may, under the specific
circumstances prevailing, maximize the demonstration effect.

"There are likely to be various pressures for disclosure of a weapon
or weapons system which have nothing to do with demonstration pur-
pose but which will implement the considerations in favor of demon-
strative disclosure. These pressures must be carefully considered to
determine whether the alleged advantages to be gained are real or
fictitious."

SOME NEEDED RESEARCH

We are not going to attempt an exhaustive catalog of research or to specify any project in detail but rather to suggest the kind of research activity that would help us toward competence and understanding in this area.

In the first place, do we not need, somewhere in this country, a centering of research on Cold War problems and strategies? It is not implied that no such work is in progress within the government or outside. Nor is it implied that the RAND Corporation and the new program at the Naval Ordnance Training Station, among others, have not contributed in an important way to the understanding of Cold War problems. But RAND has a charter much larger than the Cold War, and a specific client, the Air Force, which has its own set of problems. NOTS also has a specific client, the Navy, and is concentrating on the problem of deterrence. Somewhere in our plans for relating research to policy is there no place and need for a research organization that will have the government as its client and all Cold War relationships and policies as its concern?

We are not suggesting that another very large organization—another Manhattan Project, or even another RAND—need necessarily be created for this purpose. No such center could possibly do all the kinds of research needed to illuminate the policy problems of the Cold War; and to try to have all such research done at one place would impoverish the intellectual resources available for the task and slow the production of knowledge. A preferable arrangement would be close communication and cooperative planning between several centers of different kinds in different places. But even such a multiple body would need a single head. Therefore, it seems evident that there is need for a research organization to work and think in the broadest way on Cold War problems and strategies, to coordinate and summarize existing research and bring it to the attention of both government and other researchers, to have equal entree to the government and the research community to conduct and sponsor research as necessary, to conduct political "games," and to maintain continuing seminars and study sessions on the urgent problems of the times.

Proceeding from the most general toward the specific, let us record that we need a serious effort to construct a general theory of conflict and a general theory of decision-making. Schelling (93) has raised many of the essential questions to be answered with respect to a theory of conflict; Snyder (104) has raised many of those involved in a theory of decision-making. It may prove possible now to make general theories of these kinds, but the mere effort to do so, with the necessary gathering of evidence, construction of postulates, and testing of hypotheses, would set us far ahead in our attempt to understand these difficult and important areas.

We need also a great deal of specific knowledge about decision-making. For one thing, we need to know far more than we now do about the Soviet and Chinese patterns of decision-making and risk taking. We need to know more about decision-making under threat. In particular we need to know more about Soviet and Chinese responses to threat. Is it possible, for example, as Milburn (69) has suggested, to construct a typology of threats as they exist for the decision-makers of these countries—from the harmless to the irritating to the infuriating to the intolerable? We also should like

to know more about what the important decision-makers know about us and
our policies, how they are informed, and what are their personal qualities
and characteristics. In other words, we need to know as fully as possible
the conditions under which the important decisions that concern us are go-
ing to be made.

It goes without saying that the NOTS studies of deterrence promise to
be valuable, and should be implemented as necessary.

One much needed, large-scale activity, as McRae suggests in pages 188
to 224 below, is the building of human qualities into gaming. That is, we
need to use games to illuminate different patterns of decision-making, risk
taking, and strategy selection engaged in by different kinds of individuals
under different conditions. This would be an extremely useful kind of study
and also might enable us to complicate and revise game theory so that it
would better represent human behavior. Game theory was made in the
classical model. That is, it was made by simplifying life until it could be
put into mathematical terms and gathered into the neat pattern of a mathe-
matical theory. This required the assumptions of known alternatives,
known payoff, and so forth, which are not often reproduced in the making of
foreign policy decisions. The history of certain other theories has been
that they were developed from their early simple form to a form more
nearly in accord with human experience. It is conceivable that this might
happen with game theory, which, even as it is, is a powerful and elegant
intellectual tool.

It seems to us a good investment to experiment with playing out some
of the likely future problems of the Cold War in political games. Guetzkow's
(42) game is very good, but highly general. For playing out such problems
as new military disclosures, new and threatening troop movements, threats
to some of our overseas installations, and so forth, the RAND game, or
some variation of it, might prove more useful (23). This is essentially a
war game, played with competing "governments" and a board of judges act-
ing as Fate, Law, and Reason. These games should be excellent training
for policy-makers and perhaps useful devices for screening the next gen-
eration of policy-makers. If realistic enough, these games might help us
sharpen our strategy.

A great deal of information on audiences and their attitudes, beliefs,
values, symbol systems, and communication patterns is required. Much
of this is available but is not systematized—at least not in unclassified
form.

We need also a continuing series of studies (perhaps along the pattern
of Harold Lasswell's World Attention Survey of World War II) on what dif-
ferent countries are being told about us, especially about our military capa-
bilities and intentions. When do we make news with our military postures,
and who hears the news? What gets reported correctly, and what incorrectly?

We could benefit also from a series of case studies on the effects of
different military postures, in different political relationships, in the past.
North (115) has been studying in this way the advent of a war. We need
also to know (a) when and under what conditions deterrence has worked,
(b) when it has not worked, and (c) what kind of deterrence has been used
in each case. We need to know what kind of news treatment and what kind

of impact have resulted from different kinds of military developments and disclosures in peacetime. We need to know the "decay time," given specified conditions, of unfavorable events, such as Hungary was for the Soviet Union.

It is important also to try to project as accurately as possible the civil problems that would follow a nuclear attack on the United States (7, 54). The disaster studies and the estimates of destruction from heavy missiles with nuclear warheads make it seem likely that the country would require a very long time to reorganize itself and "come back." For a time our traditional patterns and concepts of government and business might be unworkable and our defense greatly hampered.

Without bringing into this discussion the details of problems of civil defense programs in communities and by individuals, it should be pointed out that preparation for defense against attack is itself a military posture in peacetime and one that is subject to the most delicate considerations of national morale and international interpretation. Policies relating to preparation for defense should therefore be set and adjusted in the light of the best possible information and the most realistic possible projection of need.

Finally, inasmuch as we shall be dealing with human behavior in other cultures, it is important that some of the key experiments on which we base our strategy and tactics of communication and persuasion should be repeated in appropriate places outside our own culture. These experiments probably cannot be done in the Soviet Union and China, but they can be done with Chinese people, with peoples of cultures similar to that of the Russians, and with some of the neutrals. Experiments on one- or two-sided communication, on cognitive consistency, on response to threat, and on risk taking and game strategy are the ones that immediately recommend themselves for replication in other cultures.

REFERENCES

1. Abel, T. "The Element of Decisions in the Pattern of War," American Sociological Review, 6 (1941), 853-859.

2. Abelson, R. P., and Rosenberg, M. J. "Symbolic Psychologic: A Model of Attitudinal Cognition," American Behavioral Scientist, 3 (1958), 1-13.

3. Asch, S. E. "Effects of Group Pressures Upon the Modification and Distortion of Judgments," in Groups, Leadership, and Men, ed. H. Guetzkow. Pittsburg, Pa.: Carnegie Press, 1951.

4. Backus, P. H. "Finite Deterrence, Controlled Retaliation," U. S. Naval Inst. Proc., 85 (1959), 23-29.

5. Bernard, J. "Parties and Issues in Conflict," Journal of Conflict Resolution, 2 (1957), 111-121.

REFERENCES (Continued)

6. _____ , et al. The Nature of Conflict; Studies of the Sociological Aspects of International Tensions. Paris: UNESCO, 1957.

7. Bleicken, G. D. "The Role of Non-Military Defense in American Foreign and Defense Policy," Political Science Quarterly, 74 (1959), 555-563.

8. Brodie, Bernard. "The Anatomy of Deterrence," World Politics, 11 (1959), 173-192.

9. _____ . "Military Demonstration and Disclosure of New Weapons," ibid., 5 (1953), 281-301.

10. _____ . Strategy in the Missile Age. Princeton, N.J.: Princeton University Press, 1959.

11. Brody, R. A. "Deterrence Strategies: An Annotated Bibliography," Journal of Conflict Resolution, 4 (1960), 443-457.

12. Brookings Institution. Research Frontiers in Politics and Government. Brookings Lectures, 1955. Washington, D.C.: Brookings Institution, 1955.

13. Brown, J. S. "Principles of Intrapersonal Conflict," Journal of Conflict Resolution, 2 (1957), 135-154.

14. Burns, A. L. "From Balance to Deterrence: A Theoretical Analysis," World Politics, 9 (1957), 494-529.

15. _____ . "International Consequences of Expecting Surprise," ibid., 10 (1958), 512-536.

16. _____ . "The Rationale of Catalytic War," Research Monograph, 3, 1-20. Princeton, N.J.: Princeton University, Center of International Studies, 1959.

17. Buzzard, A. W. "Massive Retaliation and Graduated Deterrence," World Politics, 8 (1956), 228-237.

18. Cantril, H. The Politics of Despair. New York: Basic Books, Inc., 1958.

19. Cooper, E., and Jahoda, Marie. "The Evasion of Propaganda," Journal of Psychology, 23 (1942), 15-25.

20. Dahl, Robert A. "Hierarchy, Democracy, and Bargaining in Politics and Economics," in Research Frontiers in Politics and Government. Brookings Lectures, 1955. (Pp. 45-69.) Washington, D.C.: Brookings Institution, 1955.

21. Daugherty, W., and Janowitz, M. A Psychological Warfare Casebook. Baltimore, Md.: Johns Hopkins Press, 1957.

REFERENCES (Continued)

22. Davison, W. P. The Berlin Blockade: A Study in Cold War Politics. Princeton, N.J.: Princeton University Press, 1958.

23. _____ . A Public Opinion Game. (RAND paper no. P-2042.) Santa Monica, Calif.: RAND Corporation, 1960.

24. DeCharms. R., and Wilkins, E. J. Studies in the Psychology of Aggression. Technical Report 5, Contract Nonr 816 (11). Chapel Hill: University of North Carolina, July 1960.

25. Deutsch, K. W. "Mass Communications and the Possible Loss of Freedom in National Decision-Making: A Possible Research Approach to Interstate Conflicts," Journal of Conflict Resolution, 2 (1957), 200-211.

26. Deutsch, Morton. "An Experimental Study of the Effect of Threat Upon Interpersonal Bargaining." New York: Bell Telephone Laboratories, 1960. (Mimeo.)

27. Easton, D. "Limits of the Equilibrium Model in Social Research," American Behavioral Scientist, 1 (1956), 96-104.

28. Festinger, L. A Theory of Cognitive Dissonance. Evanston, Ill.: Row, Peterson and Co., 1957.

29. Finan, J. L. Fear of German Weapons. Washington, D.C.: Army Service Forces, War Department, 1943.

30. Frank, J. D. "The Great Antagonism," Atlantic Monthly, 202 (1958), 58-62.

31. Frankel, J. "Towards a Decision-Making Model in Foreign Policy," Political Studies, 7 (1959), 1-11.

32. Freud, S. Why is War? (In Collected Papers, 5.) London· Hogarth Press, 1950.

33. Garthoff, R. L. "Soviet Doctrine on the Decisive Factors in Modern War," Military Review, 39 (1959), 3-22.

34 George, A. L. Propaganda Analysis: Inferences Made from Nazi Propaganda in World War II. Evanston, Ill.: Row, Peterson and Co., 1959.

35. Gillen, J. "Research Suggestions Regarding Some Policy Relatior ships of Major U. S. National Goals to Those of Other Cultures." China Lake, Calif.: U. S. Naval Ordnance Research Station, 1959. (Mimeo.)

36. Gladstone, A. I. " ·e Conception of the Enemy," Journal of Conflict Resolution, 3 (19), 132-137.

REFERENCES (Continued)

37. _____ . "The Possibility of Predicting Reactions to International Events," Journal of Social Issues, 11 (1955), 21-28.

38. _____ , and Taylor, Martha A. "Threat-Related Attitudes and Reactions to Communications About International Events," Journal of Conflict Resolution, 2 (1958), 17-28.

39. Goldhamer, H. (with the collaboration of A. L. George and E. W. Schnitzer). Studies of Prisoner-of-War Opinions on Weapons Effectiveness (Korea). (RAND memorandum no. RM-733.) Santa Monica, Calif.: RAND Corporation, 1951.

40. Goldschmidt, W., and Edgerton, R. B. "A Picture Technique for the Study of Values," American Anthropologist, 64 (1961), 26-47.

41. Guetzkow, H. "Isolation and Collaboration: A Partial Theory of International Relations," Journal of Conflict Resolution, 1 (1957), 48-68.

42. _____ . "A Use of Simulation in the Study of Inter-Nation Relations," American Behavioral Scientist, 4 (1959), 183-191.

43. Heider, F. The Psychology of Interpersonal Relations. New York: John Wiley & Sons, 1958.

44. Helson, Harry. "Adaptation-Level as a Basis for Quantitative Theory of Frames of Reference," Psychological Review, 55 (1948), 297-313.

45. Hoggatt, A. C. "An Experimental Business Game," American Behavioral Scientist, 4 (1959), 192-203.

46. Hovland, C. I., et al. "Assimilation and Contrast Effects in Reactions to Communication and Attitude Change," Journal of Abnormal and Social Psychology, 55 (1957), 244-252.

47. _____ , et al. Communication and Persuasion. New Haven, Conn.: Yale University Press, 1953.

48. Hull, C. L. Principles of Behavior. New York: Appleton-Century, 1943.

49. Hungerford, J. M. The Exploitation of Superstitions for Purposes of Psychological Warfare. (RAND memorandum no. RM-365.) Santa Monica, Calif.: RAND Corporation, 1950.

50. Janowitz, M. The Professional Soldier: A Social and Political Portrait. Glencoe, Ill.: The Free Press, 1960.

51. Johnson, D. M. The Psychology of Thought and Judgment. New York: Harper and Bros., 1957.

52. Kahn, Herman. "How Many Can Be Saved?" Bulletin of the Atomic Scientists, 15 (1959), 30-34.

REFERENCES (Continued)

53. _____ . On Thermonuclear War. Princeton, N.J.: Princeton University Press, 1960.

54. _____ . Report on a Study of Non-Military Defense. (RAND report no. R-322 RC.) Santa Monica, Calif.: RAND Corporation, 1958.

55. Kaplan, M. "The Calculus of Nuclear Deterrence," World Politics, 11 (1958), 20-43.

56. _____ . The Strategy of Limited Retaliation. Policy Memorandum 19, 1-22. Princeton, N.J.: Princeton University, Center of International Studies, 1959.

57. Katz, D. (ed.). Attitude Change. A special issue of the Public Opinion Quarterly, 24 (1960), 2.

58. Katz, E., and Lazarsfeld, P. F. Personal Influence: The Part Played by People in the Flow of Mass Communications. Glencoe, Ill.: The Free Press, 1955.

59. Kelman, Herbert C. "Compliance, Identification, and Internalization: Three Processes of Attitude Change," Journal of Conflict Resolution, 2 (1958), 51-60.

60. _____ . "Societal, Attitudinal, and Structural Factors in International Relations," Journal of Social Issues, 11 (1955), 42-56.

61. Kerr, Clark. "Industrial Conflict and Its Mediation," American Journal of Sociology, 60 (1954), 230-245.

62. Leghorn, R. S. "The Problem of Accidental War," Bulletin of the Atomic Scientists, 14 (1958), 205-209.

63. Luce, R. D., and Raiffa, H. Games and Decisions: Introduction and Critical Survey. New York: John Wiley & Sons, 1958.

64. Mack, R. W., and Snyder, R. C. "The Analysis of Social Conflict--Toward an Overview and Synthesis," Journal of Conflict Resolution, 1 (1957), 212-248.

65. Madariaga, S. de. Disarmament. New York: Coward-McCann, 1929.

66. McClelland, D. C., et al. The Achievement Motive. New York: Appleton-Century-Crofts, 1953.

67. Mershon National Security Program. Accidental War: Some Dangers in the 1960's. (Mershon Research Paper.) Columbus: Ohio State University, 1960.

68. Michael, D. N. "Civilian Behavior Under Atomic Bombardment," Bulletin of the Atomic Scientists, 11 (1955), 173-177.

REFERENCES (Continued)

69. Milburn, T. W. "A Model and Design for the Study of Deterrence."
China Lake, Calif.: U.S. Naval Ordnance Research Station, 1960.
(Mimeo.)

70. _____. "Some Logical and Psychological Considerations for
an Effective Policy of Deterrence." Paper read to American Psycho-
logical Association, 1960. (Mimeo.)

71. _____. "What Constitutes Effective Deterrence?" Journal of
Conflict Resolution, 3 (1959), 134-145.

72. Morton, L. "The Decision to Use the Atomic Bomb," Foreign Af-
fairs, 35 (1957), 334-353.

73. Nash, J. F. "Two-Person Cooperative Games," Econometrica, 21
(1953), 128-140.

74. Newcomb, T. N. Social Psychology. New York: Dryden Press, 1950.

75. Nitze, P. H. "Atoms, Strategy, and Policy," Foreign Affairs, 34
(1956), 187-199.

76. Osgood, C. E. Graduated Reciprocation in Tension-Reduction: A
Key to Initiative in Foreign Policy. Urbana, Ill.: Institute of Com-
munications Research, 1960.

77. _____. Motivational Dynamics of Language Behavior. (Ne-
braska Symposium on Motivation.) Lincoln: University of Nebraska
Press, 1957.

78 Pepitone, A., et al. "Some Reactions to a Hypothetical Disaster,"
Journal of Abnormal and Social Psychology, 51 (1955), 706-708.

79. Pool, I. de S. "Public Opinion and the Control of Armaments,'
Daedalus, 89 (1960), 984-999.

80. _____, and Gleicher, D. "American Goals and Values."
Cambridge, Mass., 1960. (Mimeo.)

81. Rapoport, D. C. Toward a Theory of Comparative Military-Political
Types. New York: Columbia University Press, 1961.

82. Reusch, H. Top of the World. New York: Pocket Books, Inc., 1951.

83. Richardson, Lewis F. Generalized Foreign Politics. Cambridge,
England: Cambridge University Press, 1939.

84. _____. Mathematical Psychology of War. London: British
Copyright Libraries, 1919.

85. _____. "Threats and Security," in Psychological Factors of
Peace and War, ed. T.H. Pear. (Pp. 219-235.) London: Hutchinson
& Co., 1950.

REFERENCES (Continued)

86 Schanck, R. L. "A Study of a Community and Its Groups and Institutions Conceived of as Behavior of Individuals," Psychological Monographs, 43, No. 2 (Whole No. 195), (1932), 6.

87. Schein, Edgar H. "Reaction Patterns to Severe, Chronic Stress in American Army Prisoners of War of the Chinese," Journal of Social Issues, 13 (1957), 31-40.

88. Schelling, T. C. "Bargaining, Communication, and Limited War," Journal of Convlict Resolution, 1 (1957), 19-36.

89. _____ . Nuclear Weapons and Limited War. (RAND paper no. P-1620.) Santa Monica, Calif.: RAND Corporation, 1959.

90. _____ . The Reciprocal Fear of Surprise Attack. (RAND paper no. P-1342.) Santa Monica, Calif.: RAND Corporation, 1958.

91. _____ . "The Retarded Science of International Strategy," Midwest Journal of Political Science, 4 (1960), 2.

92. _____ . The Strategy of Conflict. Cambridge Mass.: Harvard University Press, 1960.

93. _____ . "Towards a General Theory of Conflict Applicable to international Relations." Paper delivered at conference on international relations, Northwestern University. 1959. (Mimeo.)

94. Schramm, W. The Process and Effects of Mass Communication. Urbana: University of Illinois Press, 1954.

95. Scodel, A., et al. "Some Personality Correlates of Decision Making Under Conditions of Risk," in Decisions, Values, and Groups, ed. D. Willner. (Pp. 37-49.) New York: Pergamon Press, 1960.

96. _____ , et al. "Some Descriptive Aspects of Two-Person Non-Zero-Sum Games," Journal of Conflict Resolution, 3 (1959), 114-119.

97. Scott, J. P. Aggression. Chicago: University of Chicago Press, 1958.

98. Shubak, M. (ed.). Readings in Game Theory and Political Behavior. Garden City, N.Y.: Doubleday, 1954.

99. Simon, Herbert A. "Recent Advances in Organization Theory," in Research Frontiers in Politics and Government. Brookings Lectures, 1955. (Pp. 23-44.) Washington, D.C.: Brookings Institution, 1955.

100. Singer, J. D. "Raising the Sights of Research on Peace," American Behavioral Scientist, 4 (1961), 11-13.

101. _____ . "Threat-Perception and the Armament-Tension Dilemma." Journal of Conflict Resolution, 2 (1959), 90-105.

REFERENCES (Continued)

102. Snyder, G. H. "Deterrence and Power," Journal of Conflict Resolution 4 (1960), 163-178.

103. _____. "Deterrence by Denial and Punishment," Research Monograph, 1, 1-39. Princeton, N.J.: Princeton University, Center of International Studies, 1959.

104. Snyder, Richard C. "Game Theory and the Analysis of Political Behavior," in Research Frontiers in Politics and Government. Brookings Lectures, 1955. (Pp. 70-103.) Washington, D.C.: Brookings Institution, 1955.

105. _____, and Paige, G. D. "The United States Decision to Resist Aggression in Korea," Administrative Science Quarterly, 3 (1958), 341-378.

106. Solomon, L. "The Influence of Some Types of Power Relationship and Game Strategies Upon the Development of Interpersonal Trust," Journal of Abnormal and Social Psychology, 61 (1960), 223-230.

107. Sontag, R. J. "The Last Months of Peace," Foreign Affairs, 25 (1957), 507-524.

108. Speier, Hans. German Rearmament and Atomic War. Evanston, Ill.: Row, Peterson and Co., 1957.

109. _____. Soviet Atomic Blackmail and the North Atlantic Alliance. (RAND paper no. P-1094.) Santa Monica, Calif.: RAND Corporation, 1957.

110. Spence, K. "Theory of Emotionally Based Drive and Its Relation to Performance in Simple Learning Situations," American Psychologist, 13 (1958), 131-141

111. United States Strategic Bombing Surveys. Washington, D.C.: Government Printing Office, 1946 and following.

112. Von Neumann, J., and Morgenstern, O. Theory of Games and Economic Behavior. (2nd ed.) Princeton, N.J.: Princeton University Press, 1947.

113. Willis, R. H., and Joseph, M. L. "Bargaining Behavior," Journal of Conflict Resolution, 3 (1959), 102-113.

114. Wright, Q. A Study of War. Chicago: University of Chicago Press, 1942.

115. Zinnes, D., et al. "Capability, Threat, and the Outbreak of War," in International Politics and Foreign Policy, ed. James N. Rosenau. New York: Free Press of Glencoe, 1961.

IV

THE INTELLIGENCE FUNCTION

Klaus Knorr

Intelligence is an operation for procuring and processing information about the external environment in which an organization--in our case, the Government of the United States--wants to maximize the net achievement of its various goals.

Obviously, intelligence is concerned with the relevant future and past as well as current, properties of the external environment. Foreknowledge of relevant changes in the outside world is needed so that policy can be formulated in time to meet those changes. Moreover, our actions or policies can take effect only with varying time lags, often measured in years. First, it takes time to formulate and implement policy; and implementation often requires changes in specific financial, economic, technological. or military capabilities. Second, it may take time for implemented policies to yield cumulative effects and achieve their objectives. Prediction is therefore a central concern of intelligence. On the one hand, policy success depends upon the anticipation of external pressures and opportunities that it will be necessary to meet or advantageous to exploit. On the other hand, policy success also depends upon comparative estimates of the degree to which available policy alternatives are likely to meet such pressures and exploit such opportunities.* It is to be noted that the estimates required are net estimates of all important consequences of the policy alternatives since these may entail side-effects which--in addition to manipulating the particular outside events that are in immediate focus--may (in toto and over time) either impair or enhance our position to maximize overall goal achievement.

An estimate of future situations in the outside world is a direct operational requirement of intelligence. Concern with past states of affairs in

---

Born in Germany in 1911, Klaus E. Knorr, PhD, is Director of the Center of International Studies and Professor of Economics at Princeton University. In recent years he has been a delegate to UNESCO conferences on atomic energy and social science research in Paris and Prague, and a Consultant to the Department of State and the Committee for Economic Development. Currently he is a Consultant to the Department of Defense, the Institute for Defense Analyses, and the RAND Corporation, and a member of a committee of the National Planning Association on Security through Arms Control.

---

*In the actual organization of intelligence production, the function of predicting changes in the external environment to be induced by our own actions is usually left to the policy-maker.

the outside world is subsidiary, but knowledge of both past and current situations permits the discovery and analysis of trends that are useful to forecasting.

LIMITS OF THE PRESENT STUDY

The present study deals only with limited aspects of intelligence.

This study concentrates on the predictive function of intelligence. To be sure, a very large proportion of intelligence work is formally directed to estimates of existing situations--military capabilities, political patterns, economic pressures, etc. But to aid the policy-maker in coping with future events is usually the ultimate purpose of these estimates. In any event, the case for social science contributions to the merely analytical part of intelligence is contained in the case for such contributions to its predictive function.

This study is not concerned with the consumption of intelligence. The communication of intelligence products to policy officials and the proper integration of intelligence in policy-making are important problems, perhaps more important than those of intelligence production, but they are analytically separable from the latter.

It is possible for good intelligence to be rejected or ignored by policy officials. Indeed, there is some indication that the impediments to the proper use of intelligence are greater than those associated with intelligence production. This is especially true at a high level of policy-making (cf. Sprout, 38, page 321). No doubt social scientists--e.g., those studying problems of organization and decision-making--could contribute to solving the problems encountered in the use of intelligence. But these problems lie outside the scope of the present survey.

This study disregards certain sources and techniques of intelligence production, such as clandestine intelligence and intelligence derived from the use of trial balloons designed to facilitate an estimate of foreign responses to our action. Though these operations are susceptible to improvement by resort to social science techniques, they are especially inaccessible to outside study.

With a few exceptions that will be noted below, this study is not concerned with problems of organization and management of intelligence production.*

---

*The quality of the intelligence product is indubitably dependent on the quality of administration and management in the producing organization, and is influenced by problems of inter-agency rivalry resulting from the distribution of intelligence-gathering--and, to some extent, of intelligence evaluation--over separate government organizations, that, in many cases, have a particular and competitive consumer interest. Social scientists who study pertinent problems of management and bureaucratic organization could presumably contribute to the solution of these difficulties, but this also is a general matter excluded from this paper.

When we inquire into the influence--especially the potential influence--
of social science research on intelligence production, we refer to the fol-
lowing social sciences: political science, sociology, anthropology, eco-
nomics, and, in part, social psychology and history (though many historians
are apt to protest against the inclusion of their discipline). And we refer
to intelligence that focuses on the same subjects as social scientists do --
political, social, economic, and cultural life. (This would exclude intelli-
gence work requiring the expertise of the natural scientist, engineer, mil-
itary man, etc.)

MATERIALS FOR THE PRESENT STUDY

This study is based entirely on unclassified literature. The literature
dealing directly, and more than marginally, with intelligence production in
the United States or elsewhere is exceedingly small. There are the books
by Hilsman (16), Kent (19), Pettee (30), Platt (31), and Ransom (34),* and a
few articles, notably those by Kendall (18), Lasswell (22), Millikan (28),
and Wasserman (39).

Two conditions probably account for this small amount of public liter-
ature: first, the inaccessibility of government intelligence operations to
the academic scholar and, second, the recent origin of modern intelligence
production. Until World War II, the sources of intelligence were predomi-
nantly clandestine, whereas now clandestine sources--insofar as they do
not duplicate data available through open sources--provide only a small
proportion of information.**

Much of this slim literature on intelligence is more or less obsolete,
to the extent that it criticizes current production of intelligence in the
United States Government. The books by Pettee and Kent were published
in 1946 and 1949 respectively; and the data for the Hilsman book were
gathered several years before it was published in 1956. It is reasonable
to assume that, at its inception, modern production of intelligence had to
be developed almost from scratch, that it was initially subject to various
inefficiencies, and that the learning process, internal to the intelligence
organizations, led to considerable improvements over time.

---

*For other writings, see the bibliography in Ransom's book, pages
233-256.

**The development of modern intelligence in the United States can be
said to have begun with the establishment in 1941 of the research and in-
telligence branch of the Office of Strategic Services headed by William J.
Donovan. It was Donovan's conception that the rigorous training that so-
cial scientists and historians had received in the handling of evidence
made them indispensable in estimating capabilities and trends in enemy
countries. And the academic contribution of research techniques was in-
deed so crucial to the success of this branch of OSS that, impressed by
OSS competition, the intelligence organizations of the armed services be-
gan forthwith to recruit academic talent. Cf. Ransom (34, page 68).

PAST INFLUENCE OF THE SOCIAL SCIENCES
ON INTELLIGENCE WORK

One way of inquiring into the contributions that social science research might make to the improvement of intelligence production, now and in the future, is to estimate the contributions that social scientists have made in the past.

Unfortunately, no precise appraisal can be made. No doubt, social science has influenced intelligence production very considerably; indeed as now practiced, intelligence is inconceivable without the social sciences. But except in very general ways it is hard to identify, measure, and evaluate this influence, and with rare exceptions it is difficult to specify particular researches or research methods in the social sciences that have had a traceable impact on intelligence work.

There are two major channels through which the research capabilities and results of the social sciences are transmitted to the intelligence community. One is through the recruitment of intelligence personnel, most of whom it can be assumed, have become acquainted to some degree with the social sciences in college, and many of whom have done graduate work in a social science.* The other channel is such contact with the social sciences as intelligence staffs maintain by employing social science consultants and, above all, by reading social science work published in journals and books.

Although intelligence documen. are seldom adorned with footnote references to social science materials, it would be possible--in principle, by means of content analysis--to gain a clearer conception of the impact of social sciences on intelligence. One could, for instance, study the frequency with which social science concepts are employed in intelligence documents and even get some inkling of the tir e lag before new concepts are apparently absorbed. In practice, the worth of such a study would be somewhat limited by the fact that modern intelligence work has been conducted in the United States for less than two decades. Within this period, it should be fairly easy to trace, for example, the incorporation of terminology that economists have developed for national income analysis or that other social scientists have developed for general systems analysis, or Soviet studies, and so forth. However, even if this were done to the extent that is feasible, one would obtain only an index of impact--not an index of the value of this impact on intelligence work.

It is not surprising that the past influence of the social sciences on modern intelligence should have been extraordinarily diffuse, but nevertheless crucial. Both social scientists and intelligence personnel are engaged in procuring and organizing knowledge about roughly the same range of events for a purpose. Though the purposes of the two groups differ, at least in part, social scientists study many of the subjects with which intelligence is most vitally concerned: various aspects (a) of the capabilities, practices, and objectives of states in international affairs and (b) of the

---

*It would be interesting to ascertain the educational background of intelligence officers, especially the proportion of them who received graduate training in the various social sciences.

domestic structures and functions (whether political, social, or cultural) with which these international capabilities, practices, and objectives are reciprocally related.

Intelligence officers as a group, however, do not express enthusiasm about social science contributions to their past or future work, as Hilsman (16, page 79) points out. Many tend to regard the social sciences as "academic," using this term in a distinctly pejorative sense. They seem to ssume that intelligence and the social sciences have little in common. In view of this disposition, it seems worth ascertaining, as best we can, to what extent, and in what ways, the operations of social scientists and intelligence officers differ and coincide. We should then be in a better position to inquire into the benefits that intelligence work can derive from the social sciences.

THE NATURE OI . ' CIAL SCIENCE

There are obviously major and numerous differences between and among the practices of social scientists, historians, and intelligence officers (some of the latter themselves are former social scientists or historians). In distinguishing among their operations, I cannot, and need not, do justice to all the varieties of activities involved.

Two introductory points about social science capabilities are, however, worth making. The value of social science contributions to intelligence, or to other government operations, should not be shrugged off simply because a considerable proportion of social science output is of poor quality, in the sense that the empirical work and analysis are shoddy; or because it often exhausts itself in the mere manufacture of jargon; or because it arrives at generalizations that are vacuous and theories that are amorphous. The prevalence of deficiencies does indicate a problem. It takes a far-ranging and sophisticated understanding of the social sciences to separate the good and useful output from the bad and irrelevant; and it is hard to acquire this degree of familiarity and make it operationally available at the consumption end.

Another obstacle to appreciating the nature and values of social science output is that many actual or potential consumers of intelligence and other government work made their acquaintance with the social sciences at the college level--an encounter that may have been a superficial one for various reasons--and did so a decade or more ago. In recent years, however, the social sciences have made great progress in terms of conceptual richness and procurement of data. Thus, a person who was exposed to introductory courses in economics and sociology before the war may be handicapped by an outdated as well as a shallow conception of the current capabilities of these disciplines.

Two Parts of Social Science Work

For purposes of this study, most of the work in the social sciences can be separated into two parts: (a) the organization and reporting of

information and (b) the interpretation of information, especially for purposes of generalization.*

Over a wide range, the organization of data by social scientists is clearly relevant to intelligence work. There are academic economists who study the rates of Soviet economic growth and political scientists who study the pattern of political life in Indonesia or Nigeria or some other country; and there are intelligence specialists who are concerned with the same information and presumably keep track--at least, to some extent--of the relevant academic output.

Much of the information produced by social scientists is of immediate use to intelligence, even to the extent that social scientists do not generate information about aspects of the environment that are of prime concern in intelligence. However, it is social science methods of gathering data, of deducing data from other data, and of establishing the validity of data that are of particular value--in principle at least--in producing appropriate kinds of information for intelligence. Especially during the last two decades, social scientists have made remarkable progress in developing statistical analysis, econometrics, a variety of survey techniques, content analysis, interviewing, and other ways of obtaining data (Cf. Festinger and Katz, 14). For example, the books by Nathan Leites (24) and Alexander George (15) show how much can be learned through an ingenious application of content analysis (cf. Pool, 32); and the studies by Abram Bergson and Hans Heymann, Jr. (3) demonstrate the value of statistical methods and national-income analysis to the estimation of Soviet economic capacity. To be sure, there are aspects of intelligence for which such techniques are of no avail (we cannot subject statesmen to rigorous interviewing techniques); and even where they are applicable in principle, the pressure of time and other constraints will often militate against their systematic employment in intelligence work. Nevertheless, these kinds of social science techniques, which can be expected to be improved by further refinement and innovation, should be regarded as valuable assets in intelligence work. Were they fully appreciated as such, they would no doubt be exploited more than they have been hitherto.

The value of the social sciences is regarded with stronger skepticism, and heavily discounted, in the intelligence community when it comes to the second type of activity--interpretation and generalization. This is surprising to many social scientists, for the use of new conceptual structures often permits new insights to be drawn from available information. Thus, the studies of Arthur Burns (6) and George Modelski (29) ingeniously resorted to systems theory in order to clarify relationships in the real world and arrive at conclusions with remarkable explanatory and predictive powers.

In general, the social scientist attempts to further the understanding of complex social phenomena by singling out certain properties of these phenomena (variables); by describing, classifying, and, if possible,

_____

*In actual fact, these two types of activities are, of course, interrelated. Not only does the study of data lead to theoretical propositions, but interest in certain hypotheses, and the way a part of reality is conceptualized, guide the search for data.

quantifying and measuring them; and by attempting to discover the inter-relationships of such variables. In other words, he reduces complex social phenomena (such as investment in a national economy or in a particular industry) to abstract models in which, by means of appropriate concepts (such as the rate of interest), relationships between variables (the rate of interest and the volume of investment) are expressed in propositions or generalizations which state that if A, then B, or if A and B, then C. Ideally, these generalizations are logically consistent and capable of being con-firmed or refuted by systematic empirical observation or by experimenta-tion. A set of interrelated propositions may lead to a complex theory (such as a theory of investment or a theory of democratic government).

Admittedly, how successful social scientists have been in discovering useful hypotheses and tested generalizations is a question that even they find difficult to answer. For one thing, achievement greatly varies among social sciences--being more impressive, for example, in economics than in political science. For another, no sensible criterion of success is readily at hand. Achievement in the social sciences may be considered deplorably low when compared with some of the natural sciences; but it may be considered high if one looks at the rapid pace of progress during the last half-century. Also, it should not be forgotten that the social sci-entist tries to cope with extremely complex phenomena. As Reichenbach (36, page 122) has said of the sociologist, his "inexactness is not the fault of his methods; it springs from his subject matter, from his dealing with complex and overlapping phenomena which do not exhibit the simple struc-ture of the motions of planets and stars, but rather recall the interwoven relationships of the weather."*

Characteristics of Theoretical Work

In connection with the requirements of intelligence, four characteris-tics of theoretical work in the social sciences are worth noting.

First, in the interpretation of data, social scientists--like all scien-tists--attempt to replace intuition and common sense (i.e., conventional and usually implicit concepts, hypotheses, and beliefs) by new and more sophisticated concepts and tested hypotheses. ·

---

*Although Reichenbach concedes that, even in natural science, causal laws hold only under ideal conditions and hence should be stated in terms of probabilities, Reichenbach exaggerates the differences between social science, on the one hand, and astronomy and meteorology, on the other. The astronomer may find it easy to predict planetary movements, but he faces a forbidding problem when he becomes concerned with dynamic changes in the universe, with its origin, evolution, and future. On the other hand, the difficulties encountered by the meteorologist fall distinctly short of those with which the social scientist must cope. Though the weather is a complex system, its changes follow well-known natural laws. Prediction is difficult because information is still inadequate in scope and, above all, because obtaining and processing information still akes too much time to perfect forecasting for the immediate future. Hov ver, these difficulties are likely to yield increasingly to the continuing p 'gress in observational and data-processing techniques.

Second, when stating that, and explaining why, two or more variables are necessarily associated--that is, with a high degree of probability--the social scientist is, in this limited sense, engaging in prediction. Indeed such prediction is the ultimate test of theory.

Third, when conceptualizing an area of interest, the social scientist, though he starts by observing real life, abstracts from reality, selecting those variables which interest him, and which--if he is skillful or lucky, or both--are critical to explaining a large number in a class of events. Yet though he is necessarily interested in a large number of cases, he does not study these in full detail. He does not focus on their concrete shape in concrete situations.

Fourth, in general, therefore, as Max Millikan (28, pages 164-165) points out, the social scientist cannot predict concrete social behavior in all its complex detail. However, if he usually confines his operational interest to abstract properties and events, he thus limits himself not because of a perverse lack of curiosity but because he realizes the untractable nature of his subject matter. He may be very curious about the real world in all its detail and about its future, but he usually prefers to stay away from prediction of concrete events because he is aware of his inability to make such predictions reliably. Resort to abstraction increases the generality, and in some sense even the reliability, of his knowledge, but makes this knowledge, because it is abstract, also seem less useful to the policy-maker or intelligence officer.

HISTORY AND INTELLIGENCE WORK

At first sight, then, there seems to be a world of difference between the theorizing social scientist and the intelligence officer. The former abstracts from much of real life and favors aspects of reality that have broad properties, lead to the isolation of a few variables which, hopefully, are critical and yet easily managed, and permit generalizations about their relationships, perhaps within the construct of a mathematical model. The intelligence officer, on the other hand, apparently cannot escape the confusions of real life, is said to be usually concerned with unique events, and must report on and forecast single events rather than make very limited predictions about classes of events. No wonder the intelligence officer often asks himself whether he does not have more in common with the conventional historian, who is also concerned with single and unique events, though for the purpose of description and explanation rather than for prediction. Unlike the social scientist, who compares sharply distinct--pure and ideal--types of system properties and situations (such as perfect competition versus monopoly, democracy versus dictatorship, war versus peace, victory versus defeat), is not the historian as well as the intelligence officer confronted with continuous gradations of facts and possibilities? Can the operations of the historian clarify the problems faced by the intelligence officer?

It is true that both historians and intelligence officers necessarily follow the contextual approach and explore the flow of events in their wholeness. Lasswell (20, pages 104 ff.) describes "techniques of contextuality" developed for assembling a mass of scattered facts in a whole pattern. In a manner of speaking, their orientation resembles that of the

Gestalt psychologists, who, as Morton Deutsch (11, page 185) states, try to
view the full interconnectedness of things, who treat a total and hence con-
crete situation because, in their view, events are determined, "not by iso-
lated properties of the person or his environment, but by the mutual rela-
tions among the totality of coexisting facts," in " 'the concrete situation'."
Are not both historians and intelligence officers therefore sensitive to the
interdependence of properties and events and to the uniqueness of this in-
terdependence in the concrete case, in the full complexity of real life?
And, though both must analyze a host of particular bits of information, do
they not also share the need for proceeding from analysis to synthesis?
Marc Bloch (4, page 155) says of the historian that reintegration is the ul-
timate justification of analysis and that "the knowledge of fragments, stud-
ied by turns, each for its own sake, will never produce the knowledge of
the whole ...." Do not these common characteristics explain why many
excellent intelligence officers have historical training?

Indeed, there seem to be further similarities between the jobs of the
historian and the intelligence officer. In considering a concrete case, both
must do their best with incomplete and ambiguous information. The histo-
rian can never, and the intelligence officer can rarely, create his own data,
as the social scientist can do fairly often--for example, by means of sur-
vey research and experimentation. The knowledge of the historian is me-
diate, indirect, inferential. As Collingwood (7, pages 251-252) stated, his-
tory "... is a science whose business is to study events not accessible to
our observation, and to study these events inferentially, arguing to them
from something else which is accessible to our observation, and which the
historian calls 'evidence' for the events in which he is interested." Or, as
Bloch (4, pages 54-55) says: "... knowledge of all human activities in the
past, as well as of the greater part of those in the present, is, as François
Simiand aptly phrased it, a knowledge of their tracks." Thus the historian
derives, like the archaeologist, large reconstructive inferences from
ruins, tombs, and pieces of pottery. The intelligence officer is likewise
often compelled to draw large conclusions from little evidence. Like the
historian, and indeed like the judge and the police detective, he is incapable
of observing directly many of the most crucial phenomena with which he is
concerned. He is unable to see a Gross National Product; he cannot look
into the mind of Khrushchev or de Gaulle; and--another and an important
liability--most of what he does see, he sees through the minds of others.
Like the historian, the judge, and the detective, he must learn how to eval-
uate ("cross-examine") evidence and how to infer unobserved "fac   'rom
observed data.

INTELLIGENCE WORK AND THE SOCIAL SCIENCES

However, these differences between intelligence work and history on
the one hand and generalizing activities in the social sciences on the other,
are in part more apparent than real and, on the whole, can be greatly ex-
aggerated.

Inference and Indicators

To begin with, most knowledge is inferential, including knowledge pro-
duced by social scientists.* Moreover, generalizations about relationships
between different conditions (for instance, the relationship between busi-
ness booms and rising prices) are usually inferred from indicators.** The
study of indicators often leads to the discovery of new causes (in a statis-
tical sense). Especially in economics, psychology, and sociology, a num-
ber of indicators are often represented summarily in a statistical index.
Furthermore, when it comes to the study of subjective variables such as
attitudes or motives, which are insusceptible to direct observation and
measurement, one must try to find, in expressed behavior, objective indi-
cators of these variables. As Harry Eckstein (pages 134-138, below)
points out, the study of internal war is bound to be largely symptomatology,
concerned with the discovery of symptoms indicating "with a high degree
of accuracy, that internal war will occur in a society's future." Eckstein
further notes that the reason for this research strategy is that, though
symptoms are not causes, symptoms indicate, or are presumed to indicate,
causes that are less susceptible to direct observation and measurement.

The indicators employed by social scientists are rarely perfect in-
struments for purposes of inference. Any particular indicator is likely to
be very crude. It may indicate a variety of different, though related,
events instead of just one, and the social scientist is often forced to com-
bine two or more indicators in order to obtain a better inferential approx-
imation, thus facing the difficult problems of weighting indicators—not only
at one time, but over a period of time. Moreover, in the social sciences,
too, information on the indicator· is frequently fragmentary and unreli-
able. There is indeed a considerable social science literature on index
ambiguity, index instability, and so forth.*** The research leap from poor
data is often as bold and risky in the social sciences as the intelligence
leap from inadequate information. What seems to distinguish the intelli-
gence officer and historian sharply from the social scientist in this respect
is not the inferential use of indicators but the fact that the analytical social
scientist, as already suggested, is usually searching for useful indicators
of classes of events, whereas the historian and intelligence officer are im-
mediately concerned with indicators of specific occurrences. However,
over a wide range of political, economic, and social events, the indicators
discovered and tested by social scientists are--whenever the supply of data
permits--of immediate utility to the intelligence officer; and there is in
fact considerable use in intelligence work of economic indices, content
analysis, public opinion polls, etc.

Moreover, and more important, the concern of historians and intelli-
gence officers with the single event and its uniqueness is also subject to
exaggeration and misunderstanding. It is, of course, true that the focus of

_____

*In a recent book, Conquest (8) compares his task of unraveling the
complexities of the Soviet power struggle with a paleontologist's recon-
struction, from mere footprints, of the characteristics of the chirotherium.
**For an interesting example of the search for indicators in political
science, see an article by Karl W. Deutsch (9, pages 49 -497).
***Cf. Lazarsfeld and Rosenberg (23, passim). The bibliographical
notes are especially valuable.

intelligence officers and historians is on particular events and that every
particular event or situation is indeed unique. But, in the observer's un-
derstanding, an event becomes non-unique when, in the process of under-
standing, it is relegated to membership in a class of events or situations.
The aim of historians and intelligence officers is precisely to <u>understand</u>
particular events, and such understanding inevitably involves comparison
and hence classification and abstraction. Marc Bloch (4, pages 146, 147)
affirms that nothing can be understood by itself, that understanding re-
quires comparison. And he concedes that history, like all sciences, can-
not dispense with "abstractions."

The Inductive Fallacy

Intelligence officers and historians would be in great error if they as-
sumed that "facts" speak for themselves, that their task is to assemble as
many facts as possible, ideally "all the facts," and that knowledge of many
facts assures correct understanding and prediction of particular events.
This assumption represents the "inductive fallacy," which has been casti-
gated by many critics of those intelligence doctrines that seem to subscribe
to the assumption. (Cf. Hilsman, 16, pages 51-55, 62-64; Millikan, 28,
page 163; and Wasserman, 39, pages 158 ff.) The meaning of facts is not,
of course, self-evident; it must be construed. The central task of the in-
telligence officer, historian, and social scientist is to fit facts into mean-
ingful patterns, thus establishing their relevance and bearing on the prob-
lem at hand. Without proper methods for doing so, the mere accumulation
of facts tends to be stultifying rather than enlightening. If the historian
subscribed to the inductive fallacy, he would be at best a chronicler; and,
incidentally, chronicles--with all their presentation of unorganized unique-
ness--are, of course, unpredictable. To quote Bloch (4, page 144) again:
"Like any scholar, like any mind which perceives at all, the historian se-
lects and sorts. In short, he analyzes. And, to begin with, he seeks out
the similarities in order to compare them."

The Uses of Concepts and Generalizations

Thus, neither the historian nor the intelligence officer is able to
forego categorization and do without a multitude of concepts such as lead-
ership, prestige, bureaucracy, nation, power, influence, stability, and the
like. The competent historian will not only exercise care in the selection
of the concepts he employs and possess a craftsmanlike knowledge of their
meaning but also will most likely select his conceptual frameworks with a
view to applying, implicitly or explicitly (and if he is careful, certainly
tentatively), the kinds of generalizations that social scientists are apt to
produce. In other words, in ordering and interpreting information, the
historian--like the social scientist--should eschew common-sense con-
cepts, hypotheses, and beliefs whenever there are available superior, sci-
entifically tested substitutes (which usually have been produced by scien-
tists, including social scientists). If the historian does so with due
discrimination, he is more likely not only to avoid errors of interpretation
but also to sharpen his alertness to the meaning of data and benefit from
new perspectives. It is appropriate concepts and generalizations that il-
luminate the "facts," which, being understood, have necessarily lost much
of their concreteness and uniqueness. The generalizations usually pre-
ferred by the historian are limited rather than sweeping (see 17, pages
155 ff.), but generalizations they are just the same.

The competent intelligence officer proceeds in like manner. In attempting to understand some current crisis about Berlin, he will bring to bear a great deal of knowledge about previous Berlin crises and about diplomatic crises in general. And so he will proceed when trying to predict an election in Japan, or the rate of economic growth in Communist China, or the outcome of a guerrilla war in southeast Asia. When the historian traces the French Revolution back to the social and economic conditions of the ancien régime, the crux of his intellectual task, as noted by Lazarsfeld and Rosenberg (23, page 387), "lies not in finding regularities, but in applying available knowledge to the understanding of a specific case--be it a person or a collective." In this respect, then, as Joynt and Rescher (17, pages 153-154) point out, the typical difference between social scientists, on the one hand, and historians and intelligence officers, on the other, is that the latter are consumers of generalizations whereas the former are producers of them. (It goes almost without saying that social scientists also consume generalizations and that historians and intelligence officers often produce them.) The essential point is put somewhat differently by Lasswell (22, page 2): "The generalisations of science state the conditions under which specified phenomena occur; and it is the responsibility of the projective thinker to estimate when, if ever, the requisite conditions will be found."

What intelligence work and the social sciences have in common, then is a great deal more than intelligence officers are wont to concede. Both depend on data; both require concepts and hypotheses; both are involved in comparison and hence generalization; and both are interested in prediction.

Focus on the Particular Event

On the other hand, there are also basic differences between intelligence work and the social sciences. The central difference is that the social scientist need not, and usually will not, predict particular events in a concrete setting. The intelligence officer (and the policy official), however, is forced to make such predictions even though the job is hard and success always uncertain. To predict is, after all, his profession. While the social scientist forms general hypotheses about classes of events, the intelligence officer must form particular hypotheses about a concrete stream of events. The social scientist produces science (though he is not always a producer of good science); the intelligence officer is a consumer of science (though he is not always an alert and knowledgeable consumer).

The intelligence officer's role is similar to the roles of the physician, judge, police detective, and engineer. They are all interested in particular, concrete cases. The engineer, for instance, may build a particular bridge in a particular setting. In doing so, he should know whether the bridge will withstand the load and other strains it is likely to be exposed to. He must consider simultaneously more variables than a scientist, aiming at general laws, prefers to take into account.* Similarly, the clinician who wants to decide whether a particular patient will survive a particular operation must consider variables of great complexity. Like the

---

*Even scientists, however, must extend the range of considered variables when they engage in experimental laboratory work on particular processes.

intelligence officer, he must utilize given facts, usually insufficient, and general laws, often somewhat vacuous, in order to arrive at a particular hypothesis about the case in question (cf. Meehl, 27, pages 46-50).

Social Science Contributions

These similarities and differences between intelligence work and the social sciences illumine the kinds of help that the intelligence officer can expect, and should seek, from the social scientists; this help includes social science theory. If the intelligence officer criticizes social scientists for not giving him direct answers to his professional problems, he clearly misunderstands the nature and limits of social science, and he fails to realize that intelligence work is as much an art as a science, or in large part—to put it less loftily—an engineering application of social science. What the intelligence community can expect and obtain from the social sciences are tools: data, techniques for processing and validating data, concepts for specifying required data, and generalizations that assist in interpreting data and arriving at particular hypotheses.

When the intelligence officer aims his projective thinking at a particular problem context, he must abstract irrelevant features from the concrete richness of reality. This he does at the risk of ignoring the relevant because, beyond the range of the obvious, there simply are not nearly enough good rules for distinguishing the relevant from the irrelevant.

Though, even at the present stage, the social sciences can help a great deal—indeed, more than many intelligence officers are willing to accept or look for—the tools produced by these disciplines are, of course, far from perfect or of sufficient scope. This is especially apparent when it comes to illuminating concepts and generalizations. The absence of adequate rules for deciding what is important in a concrete situation reflects in part the still relatively primitive state of social science knowledge, especially of scientific knowledge about dynamic rather than static problems, and notably about political, social, economic, military, and cultural change (Cf. Millikan, 28, pages 173-174).

The extent to which future advances in the social sciences, the discovery of new theories and indicators of change, will permit richer contributions to the performance of intelligence work is a matter of doubt. Social science branches which, relative to others, are now highly developed have a record that is both discouraging and encouraging. Although demographers can quantify their analyses to a comparatively high degree, and although they have drawn valuable lessons from the faulty population predictions of the 1930's and 1940's, they still find their forecasts upset by changes in human behavior that are hard, if not impossible, to foresee. As pointed out in an editorial of The Economist (12, pages 437-438), official British demographers in 1956 forecast the 1965 population of the United Kingdom at 52.2 million (a figure that was actually surpassed in 1960) and in 1961 expected the population figure to reach 53.7 million by 1965. The discrepancy may seem small, but it surely makes a difference to economic planning or to estimates of military strength. On the other hand, the improvement in data collection and analysis made it possible to say that Britain was experiencing a boom in babies in 1961 and thus to adjust the estimates for 1965.

Despite continuous refinement of techniques, the experience with econometric forecasting is equally mixed and may serve to explain briefly the reasons for the failure of this type of prediction. Economists (e.g., Lawrence Klein and Jan Tinbergen) have developed for such countries as the United States, Canada, Great Britain, and the Netherlands econometric models that attempt to provide, first, a quantified description of how the economy in question operates and, second, a forecast of how it will behave, in aggregate terms, in the future. The models consist of a series of specified relationships between consumption, investment, employment, wage rates, prices, taxes, and other variables; and the forecasts draw upon a battery of indicators in the form of price and wage indices, department store sales, inventory changes, etc. But the forecasts have been much less than perfect, partly because the statistical data were poor or slow in coming in, but largely because relationships between variables have been oversimplified or change over time in an unexpected fashion.

Indeed, forecasts made with the most sophisticated mathematical techniques have shown poorer results than less refined models, in part because the more sensitive the indicators are, the more likely it is that small and very short-run fluctuations are mistaken for medium-run changes. Without doubt, this experience is discouraging, especially when one thinks of the extension of such techniques to other fields of application. As a whole, however, the experience is also encouraging (and this is very important), for, as Samuelson (37, page 196) points out, these modern techniques do permit the trained economist to forecast appreciably better than the non-economist, and it is virtually certain that this capability will be improved as a result of experience and further innovation.

There are indications that the development of predictive capacity in the social and political disciplines may be on the edge of a breakthrough. Unlike the economists, political scientists and sociologists have hitherto found it difficult to deal with more than one variable at a time, and this has impeded the construction of complex models for purposes of explanation and prediction. Recourse to simulation techniques and computers will probably break these barriers and bring the capability of these disciplines to that of the economists. The further development of computers and computer programing may turn out to provide the social sciences with a powerful tool, especially for the quick checking of essentially intuitive propositions (Cf. Pool and Abelson, 33). For the same reason, and to aid in the handling of masses of data, this development may also confer direct and very important benefits on intelligence work.

SOME SPECIAL PROBLEMS OF INTELLIGENCE WORK

Intelligence work raises the central question of what kinds of events are reasonably predictable on a probability basis.* I am not aware of any systematic work on this subject.

---

*I ignore the logical problem of whether or not a single event is, strictly speaking, predictable at all. Cf. Rapoport (35, pages 90 ff., 366). Reichenbach's answer, though tortuously arrived at, seems satisfactory to me. Cf. Reichenbach (36, pages 124 ff.). See also Meehl (27, pages 19, 24-36).

Information and Prediction

To the extent that future events can be inferred at all from the data of the present, the critical conditions of successful prediction--successful in the sense that the predicted event will occur with a high degree of probability--are: first, identification of the required information, including substitute data (indicators), and, second, access to such information. On this basis, it should be possible to begin with a ranking of important kinds of events in terms of their probable predictability. Whatever the outcome of such a study, its results would require revision over a period of time in the light of future progress with data identification and procurement. The latter, of course, to the extent that it depends on clandestine sources, will always vary with luck.

The theory of identifying required data as indicators of a large variety of events is no doubt capable of improvement, in large part as a result of further progress in the social sciences, but it is doubtful that it will ever measure up to more than a good proportion of the estimating jobs in intelligence. Indicators that are good for a class of events--for instance, political revolutions--may fail in the case of an individual event whose imminence may be presaged by indicators so specific to a particular context that they will never be considered as indicators of a class of events.* By itself, moreover, the theory of indicators will hardly ever do more than specify symptoms indicative of, say, the likelihood of war or revolution. The more or less firm prediction that a war or revolution will take place at such and such a time, in such and such a manner, is a matter not for social science and theory but for intelligence; and the extent to which such predictions should be expected from the best intelligence service is a crucial question. Thus, it is possible that--in view of the alliance systems and commitments existing at the time--the probability of the outbreak of a major war was predictable in 1910 or even earlier (Cf. Aron, 2, pages 30 ff.), but it was obviously impossible to predict the extraordinary sequence of events that actually precipitated World War I.

Once the need for data has been established, procurement should improve, again partly as a result of social science ventures. But in this direction, too, a variety of constraints will set limits to what is possible. It will always be difficult to predict events which, owing to the very nature of the enterprise, are planned in great secrecy.

---

*Thus, it has been said that the sudden initiation of official surveillance of the Soviet Embassy in Belgrade presaged the break between Yugoslavia and the Bloc in 1948; and that the demonstrative march of Turkish cadets in Ankara "indicated" the imminence of the recent military coup in Turkey. A keen observer may be struck by such out-of-the-ordinary events and interpret them correctly; and in retrospect, it is easy to identify such signs of impending events. But they may not lend themselves to generalization.

Of course, even the "odd" indicator does belong to a "class." The point is that the class may never have attracted systematic attention, and thus has not been defined and studied, or the class may be so general that division into narrower sub-classes with more restricted properties is required if intelligence work is to benefit.

Even when we know what information to seek and are able to get it, our prediction may be wrong because of the intervention of a fortuitous event--a crop failure, the death of a statesman, or any of a large number of chance occurrences. The plan for a particular coup d'etat might insure a high probability of success, and, if the information were available to us, the outcome might be so estimated by intelligence. But the plan might fail and the estimate prove wrong because some unforeseeable chance event forced the premature execution of the plan. This is exactly why, even at best, intelligence prediction is contingent prediction and why the future is predictable only in terms of probability. All that can be said by intelligence is that the structure of a case favors a certain outcome, more or less, with an imputed probability. Assured prevision of the future is impossible. The consumer of intelligence should always remember this and hence cope as best he can with the inherent problem of uncertainty.

With inadequate information*--and this is a normal condition of much intelligence work--intelligence production must be regarded as all the more speculative, yielding results that can always be upset by a new scrap of evidence or by the unrolling sequence of events. If a competent intelligence service cannot do better than conclude that a particular event may have such and such consequences, with no estimate of probabilities attached, this is precisely what the consumer should expect. That many events are, for one reason or another, unpredictable is scarcely a matter for surprise.

In a recent paper on intelligence, Wasserman (39, passim) reproves American intelligence for its frequent failure to protect the policy-maker from unpleasant surprises. He sites as examples of intelligence failure the Japanese attack on Pearl Harbor, the Chinese intervention in the Korean War, the launching of Russian sputniks, and similar events. Wasserman's (39, page 156, footnote 1) definition of intelligence failure refers to the "foreseeable event which surprises those policy-makers otherwise in a position to have influenced that event." This definition fails to prove helpful since it omits an explanation of the term "foreseeable event." Referring to the prediction of international military actions, he (39, page 157) does state that they "usually occur after a period of protracted friction and military action requires lengthy preparation which cannot be concealed. It should therefore be impossible to surprise any country with an efficient intelligence service--at least they should not be surprised to the extent to which they usually are surprised. The problem which this paper seeks to explain is why governments are in practice often surprised, when in theory such surprise should not be possible." But the author forgets that periods of protracted friction and military preparation are far more frequent than outbreaks of international hostilities.

Indeed, Wasserman--whose study is mostly a repetition of the earlier criticisms by Hilsman (16) and Kendall (18)--seems to know little about

---

*"Inadequate" intelligence does not necessarily mean a scarcity of information. Often the problem is one of overabundance. For instance, several coups d'etat have occurred in the Middle East since World War II. But for every coup that occurred in that region, and was not predicted, the intelligence community probably received dozens of rumors of impending coups.

intelligence work or about the historical cases he mentions. His main thesis is that intelligence failures occur because governments do not possess or seek the sort of knowledge or intelligence necessary for sound evaluation and prediction and that they do not do so primarily because they employ inadequate conceptual frameworks, are fatally attached to the inductive fallacy, and are hostile to "intellectualism, theorizing and reasoning" (39, page 159) But exactly how competent intelligence work should proceed remains obscure in Wasserman's analysis. He certainly does not seem to appreciate the severe limits set to the prediction of future events. He does not offer a theory of intelligence but merely points up the need for one.

Judgment, Intuition, and Insight

Professional intelligence work is partly a science, an applied science; but it is also in large part an "art." Even for the bulk of its almost "routine" work, professional intelligence cannot do without judgment or, better, without intuition. For its most difficult tasks, intelligence needs intuition of a highly inventive sort. Further progress in the social sciences can be expected to reduce the area in which final reliance must be placed on intuition; this is highly important, since objective knowledge is, for most purposes, superior to introspective knowledge or "common sense." Above all, such progress--along with sheer experience--can be expected to educate and discipline the intuitive process. But over a wide range of intelligence work, and especially when confronting complex situations, the intelligence officer will in all probability rely upon intuition and ingenuity as indispensable processes. There are no "scientific" procedures for arriving at net judgments about complex social situations (Millikan, 28, page 165).

Dependence on judgment is what the intelligence officer shares with many other professionals. Lerner (25, page 13) states: "The historian, the judge, the physician follow methodological procedures that hinge upon a common characteristic: their personal judgment must play the decisive role in assembling the evidence, drawing the inference, and formulating the conclusion in each case they consider." According to Lerner (25, pages 14, 17), the essential dependence on judgment derives, as in the case of the clinician, from the need to "decide single 'cases' rather than, like the scientist, establish regularities among diverse events .... Introspection helps to close the gap between the body of evidence assembled and the inference drawn from this evidence." In the professions mentioned above, the formulation of a good particular hypothesis about a particular case is a truly creative act; and more often than not, in the context of discovery, the particular hypothesis was "suggested by the facts," as the successful practitioner will put it, rather than drawn from them by formal analysis (Cf. Meehl, 27, pages 59 ff.).

We need not pretend that this dependence on judgment is in any sense ideal, for--as in medicine--one professional's judgment may be better than another's but we cannot readily measure that of either, and the incidence of misjudgments is no doubt heavy. The crux of the matter is that resort to judgment is a necessity even though we hope that practice will improve as science and education progress and better diagnostic instruments invented.

The mind of the professional intelligence officer then should possess a radar sensitive to significant cues in the external environmert with which he is concerned. He must be able to find the "tracks" and read them right, to ask the right questions of the data, and to recognize a "fit" in a multitude of cues.

Thus there is in intelligence work, as in other professional work, a need for the truly superior mind with a gift for discerning new relationships and for recognizing the far-fetched pattern in which all the bits of evidence and impressions fall into place to produce a flash of insight. As Rapoport (35, page 259) puts it, it is "an ability to perceive hidden analogies, a tendency to juxtapose what does not ordinarily seem to belong together but can be seen to do so upon being juxtaposed." Speaking of the insight of the clinician, Meehl (27, page 88) calls it a "sort of instantaneous Gestalt-like synthesizing operation" in which no actuarial predictions are possible in view of the unique properties of the case. This sort of insight plays a striking role in all discovery--in the physical sciences and in the social sciences as well. How does one ever get any new and fruitful hypotheses?

Unhappily, as Meehl (27, Chapter 7) notes, we know very little of what constitutes, or what produces, good judgment and ingenious imagination in intelligence work or elsewhere. Possessors of the gift do not seem to rank high in terms of self-intelligibility. They may be able to verbalize but are apt to give inadequate, if not misleading, explanations of how they arrive at a good judgment or at the flash of superior insight. As Millikan (28, page 165) observes correctly, the "human brain is an extraordinary instrument, only a small portion of whose analytic powers can be reduced to communicable logic." Indeed, the human brain is like a vast, complex computer with an extraordinary memory and extraordinary powers of composition. Gifted intuition is no doubt based on a rarely well-stocked computer. But its possessor did not consciously do the programming, he can tell us little about its operation, and he finds it difficult to explicate the steps in the formation of hypotheses. This is certainly one subject on which successful research is urgently needed.

Professional Problems

In addition to the problems raised in the foregoing, there are faults of vision that tend to keep intelligence officers (and social scientists as well) from looking for the right clues or cause them to misread the clues coming their way. Faulty vision has two main determinants. First, there is the tendency--though this can be observed more frequently in the consumers than in the producers of intelligence--to see things that we want to see, to see things as we want to see them, and hence to predict the preferred rather than the probable outcome of events. It can be taken for granted that professional intelligence officers are alert to the grosser forms of this bias, but it is not clear that they have all made themselves immune to its more subtle workings.* Second, and perhaps more important, is the

---

*One member of the intelligence community told me: "Sometimes it seems to me that the worst enemy of good intelligence is patriotism. We are all prisoners of our upbringing, environment, and acquired prejudices, and I am not saying that we should be otherwise, out of office hours. But the detachment which ought to characterize the intelligence officer's work during office hours is hard to preserve."

well-known difficulty of understanding behavior that is alien to one's own culture, especially to one's own political culture, and the consequent tendency to operate with belief systems about the properties of the outside world that are stereotyped, often unrealistic, and hence apt to prejudice or distort perception.

Another problem peculiar to the intelligence officer's trade is that he is always concerned with life on the run and consequently often works under the handicap of severe time pressure. He requires a quick-reaction capability (Evans, 13, pages 89-90) that the social scientist does not need. It is the essence of this occupational predicament that the intelligence officer usually cannot put off predictions pending further information and research.

As indicated in the foregoing, a large proportion of the intelligence output is necessarily speculative, and hence permissive, and more or less plausible, but incapable of compelling assent. Largely in appreciation of this condition, but also because of the need to bring a variety of expertise to bear and because of the haste with which the estimating task must often be performed, a great deal of intelligence production takes place in formal or informal committees--far more so, incidentally, than in the case in the social sciences. Beyond doubt, this heavy recourse to the committee, to "pooling thought," is a necessity and offers considerable advantages in the form of speed, richer inputs, and checks on results. But there are also drawba.   if the pressure of meetings leaves too little time for private reflection and far-ranging reading or if the emphasis on committee work weakens the incentive for personal achievement, leads to an averaging of personal estimates, and so forth. As Allan Evans (13, page 88) remarked, the question for intelligence work is "pretty clearly not whether joint research is necessary, but whether we have gone the right way about constructing and articulating group action to that end." Should there be as much pooling of thought as there is, and is it organized to best advantage?

The control of "interest"-oriented intelligence is another problem of intelligence production. It, too, stems in large part from the fact that intelligence estimates are usually not compelling. The closer intelligence is produced, in the bureaucratic context, to the consumer in government, the more it tends to acquire a bias favoring actions preferred by each consumer group; one would expect, for example, that separate estimates of Soviet military strength undertaken by each of the three military services will tend to come to conclusions which, in each case, suggest a need for increasing the capability of the particular service in question. And when national requirements call for the integration or "coordination" of separate intelligence estimates on the same subject, the bargaining element is bound to become important and will often be reflected in the integrated estimate.

SOCIAL SCIENCE RESEARCH AND IMPROVED INTELLIGENCE

On the basis of the foregoing, it should be clear that research and training in the social sciences have made a vast contribution to intelligence production, that modern intelligence is in fact unthinkable without social science inputs, and that professional intelligence work can and will profit from further progress in the social sciences. The conceptual

structures, the repertory of research techniques, the generalizations, the indicators and data produced by social scientists, all have obvious relevance.

It is also clear to me that a strong case can be made for appreciable financial support of social science research on the part of federal agencies. This case, however, would not be confined to expected payoffs for intelligence work only, but would point to the social sciences as a general national resource capable of expansion and enrichment. Abundant funds are not, of course, the only condition of flourishing research. Nevertheless, it is difficult to doubt that progress in the physical sciences has been accelerated by the availability of relatively large funds--notably, federal funds. I am convinced that larger funds, properly administered, would also accelerate progress in the social sciences, even though--since social scientists deal with intrinsically more difficult subject matters than physical scientists usually do--one should not expect spectacular results, and one should not expect good results to be quickly a id easily recognized as such.

With one exception, however, I do not think that federal funds should be allocated to particular studies--as against undirected researches--in the social sciences as long as the object is to upgrade performance of intelligence work. This negative finding results from the views (a) that there is virtually no good research in the social sciences which does not potentially have a direct or indirect bearing on intelligence production and (b) that it would be impossible to predict exactly which particular researches are likely to have the greatest specific payoff.

It is not easy to resist a strong temptation to pick for support social science researches addressing themselves to particular research problems. Thus, it has been stated convincingly that the social sciences have hitherto favored research on static complexes to the relative neglect of dynamic situations and that researches on problems of major change--political, economic, social, and cultural--would particularly benefit intelligence production (Cf. Millikan, 28, pages 174-176, and Lasswell, 21, page 4). Obviously, a sophisticated science of social change would be a most valuable input for intelligence work. Promising progress in this direction has in fact been made by economists, demographers, sociologists, and, more recently, by political scientists as well.* Yet the subject matter for a science of social change is practically all-embracing and it is hard to specify particular researches that should be supported and others that should not. Somewhat more specifically, one might single out the study of "internal war," that is, of the resort to political violence within communities, a phenomenon which is occurring with extraordinary frequency, with which the intelligence community is deeply involved, and about which the social science literature is far from rich. But how can one study "internal war" without, indeed, producing a general science of social change? At first sight, there seem to be good reasons for supporting all social science work concerned with gathering data, especially quantitative data, developing refined techniques for doing so, and perhaps even for supporting the "continuing world survey" proposed by Lasswell (22, pages 25 ff.). Alternatively, one might single out attitude and opinion research or other

---

*Cf. Almond and Coleman (1, especially Chapter I and Conclusion).

subjects. Many specific suggestions for research are discussed in the
other chapters of this book; all are relevant for intelligence.

But one is always led back to the conclusion that there is little in so-
cial science which is not, or may not be, highly relevant to intelligence
work, even if only indirectly, and that the sensible prescription is to sup-
port social science research in general, and let the social scientists de-
cide on research subjects--a conclusion on which government administra-
tors are unlikely to be keen. Such support for undirected research also
seems desirable because there has been, over the past few years, a sub-
stantial expansion of directed research. The latter form of research,
which is usually undertaken on the basis of a contract, is focused on spe-
cial projects with the specific objective of contributing to the solution of
problems with which the government is grappling. If this trend continues,
there is a real danger that too large a proportion of social science re-
sources will be claimed for these directed purposes with the result that
the training of more social scientists and, above all, the undirected search
for new concepts and propositions, will suffer.

Toward a Theory of Intelligence

The one exception to my recommendation against directed research
that I would make, at least tentatively, is support--in the form of access to
relevant information as well as finance--for the development of a theory of
intelligence, and perhaps also a doctrine of intelligence.

The first thing the social scientist is bound to notice about intelligence
work, as conducted in the West at any rate, is that there is no satisfactory
theory of intelligence--neither a descriptive theory that describes how in-
telligence work is actually performed nor a normative theory that attempts
to prescribe how intelligence work should be conducted. There are begin-
nings and fragments of such theories, particularly in the excellent writings
of Hilsman, Kendall, Kent, and Millikan and, less explicitly, in the minds
of at least some members of the intelligence community; but a fully devel-
oped theory or set of theories does not exist. As long as it is lacking, we
have no criteria (indicators!) for judging whether intelligence work in the
United States is done well or badly or for specifying ways of improving it.
To formulate such theories would seem to be an important, though admit-
tedly difficult, undertaking.

For the development of such a theory, social scientists have valuable
skills. Some parts of such a theory, especially its normative components,
would require inputs from other sources than social science, but the social
science contribution would be crucial. In order to develop a useful theory,
social scientists would, of course, have to be granted access to the intelli-
gence world so that they could properly observe its modes of operation and
understand the demands made upon it.

A theory of intelligence must obviously deal systematically with the
major inputs--intelligence personnel, intelligence organization, the scope
and nature of information, the modes of analysis and production, and the
kinds of demands made by consumers. Descriptive theory would specify
and evaluate the properties of inputs and outputs; normative theory would
address itself to the question of what the outputs and inputs should be (to

the extent that these are subject to manipulation). The following tentative
list suggests the kinds of problems that call for study:

(a) The procurement and processing of data (including indicators);

(b) The use of hypotheses for specifying what sorts of data are de-
sirable for various intelligence tasks;

(c) The use of general hypotheses in generating particular hypotheses
about concrete situations;

(d) The determinants and uses of good judgment, intuition, and supe-
rior insight:

(e) The limits of intelligence forecasting, and criteria for ascertain-
ing the success or failure of intelligence;

(f) The types of biased vision and their minimization;

(g) The committee and bargaining aspects of intelligence production--
their uses and control.

The research required for the formulation of a theory of intelligence
would be both conceptual and empirical. Given access to intelligence rec-
ords, the researcher could do a great deal of empirical work that would
throw light on the major problems of intelligence production. For exam-
ple, systematic post-mortems of past intelligence forecasts should tell us
much about problems and approaches to them, about indicators and kinds
of information that have been relatively neglected or overemphasized,
about analytical scope and consistency, about biases, about what can and
what should not be expected from a good intelligence service, and so forth.

To give another, more problematical, example: in view of our igno-
rance about the determinants of good intuition, judgment, and insight, it
might be worth exploring whether or not it is possible to separate a sam-
ple of intelligence officers having a superior forecasting record from a
sample of officers whose predictive record is comparatively inferior.* An
interesting study might be made to determine the properties the two groups
have in common. This would be easy so far as background material on
age, education, and experience is concerned. It is, of course, possible that
resort to these easy references would fail to distinguish the two groups
because what really distinguished them in terms of success might have
resulted from factors about which we know little at present.

However, unless they can be shown to be insurmountable, difficulties
are no good reason for not undertaking researches on subjects that go to

---

*The obstacles to such a study are obviously great, partly because of
the committee nature of much intelligence work and partly because of spe-
cialization, which makes the intelligence officer usually charged with rel-
atively easy forecasting tasks look superior to officers involved in the
more difficult estimates. Once recognized, however, these difficulties
could be minimized, if not entirely overcome. And there is the further
possibility of assembling and confronting two such groups not with refer-
ence to past performance but by means of experimental gaming.

the heart of the intelligence function and might turn up extremely useful results. It is high time, it seems to be, to get on with this job.

Intelligence Doctrine

Obviously, intelligence work, as conducted by a large organization, is in need of a doctrine that defines the jobs to be done, the methods to be followed, the results to be expected, the errors to be guarded against, and so forth--in short, a strategy of intelligence inquiry. Indubitably, such a doctrine exists, though it is unlikely to be formal or more than fragmentary. A doctrine of intelligence work is not, of course, the same thing as a theory of intelligence A good formal doctrine, however, requires a good theory of intelligence work as a major input. The doctrine is the operational expression of the theory. Social scientists are able to produce a descriptive theory of intelligence and could make valuable contributions to a normative theory. To formulate doctrine is the task of the policy-maker and administrator who is informed by the theory.

I am not, of course, suggesting that--given resources, time and full access to the intelligence world--social scientists could or would be sure to come forth with a full-blown theory which would settle all problems of intelligence work. The theory would probably be fragmentary, suffering from awkward lacunae of ignorance and uncertainty and exhibiting a penchant for generalities where the would-be formulator of an intelligence doctrine would want more specific instruction. There is excellent reason to suspect, however, that--as in the case of the econometrician versus the strictly intuitive operator--the intelligence operator who is equipped with the best knowledge which the theorists have to offer is likely to do a better job than the one who relies exclusively on "experience," "common sense," "intuition," etc.

There is the further question, however, of whether or not part of an effective doctrine for an intelligence organization--indeed, for the effective working of any organization (cf. March and Simon, 26, page 151)--is not a model of reality, a world perspective that recognizes and evaluates the main revolutionary forces which, during any particular period, operate to upset the status quo. The communists have such a model. As Kenneth Boulding (5, page 24) said recently, the communists believe that "they have the key to history .... I happen to think that the communist key is a rusty one; there are many locks which it does not fit. However, a rusty key may be better than no key at all--and I recommend therefore that we give the highest priority to the occupation of key making." And social scientists have some of the equipment for making keys. Admittedly, there is the danger that, when embodied in an operational doctrine, such an overall world view becomes rigid, doctrinaire--in fact, stereotyped. But the absence of any world view may be equally stultifying. To use an analogy, Martin Deutsch (10, page 102), a physicist, said about the understanding of the physical world: "... if one is too strongly attached to one's preconceived model, one will of necessity miss all radical discoveries .... On the other hand, if one is too open-minded and pursues every hitherto unknown phenomenon, one is almost certain to lose oneself in trivia." The point is that a useful operational doctrine must always be considered tentative, subjected to continuous proof and disproof, and revised from time to time; but it can serve as a useful framework from which interpretations and forecasts of particular events can profit in depth of understanding.

The development of a theory of intelligence would be one major contribution which social scientists can render toward the improvement of intelligence. Potentially still more important is the contribution they can make by simply continuing with, and expanding, social science research-- that is, by enriching available knowledge through the relatively undirected production of data, concepts, hypotheses, and tested generalizations. It is, however, up to the intelligence community to exploit this potential by properly availing itself of what the social scientists offer. The disposition to do so is not uniformly strong, to put it mildly; and to organize the sifting and proper use of social science products, and of social scientists themselves, is indeed one subject with which intelligence doctrine must deal.

Bias Against the Social Sciences

The indifference, suspicion, and sometimes built-in hostility with which not a few intelligence officers (as well as other government officials) look on the social sciences are well known. What is not well known are the reasons for this attitude and its spread. These reasons call for study, a proposal which is bound to be resisted by many operators. One intelligence officer who read a draft of this paper said that my proposal for a theory of intelligence encountered a polite but decidedly cool reception among his colleagues; he concluded: "Who wants to have his activities dissected anyway?" Yet this coolness is far from universal. Another intelligence officer favored the proposal for a theory of intelligence and remarked: "I have long felt that the intelligence analyst is too wedded to his cables and current reports, and insufficiently au courant with what is going on generally in social science research to give him the optimum balance of inputs. He usually lacks sufficient conceptual framework into which to fit the bits and pieces which are his daily grist. To put it crudely, he ought to spend less time on newspapers and reports and more on absorbing periodical literature and books." Another reader, who has been connected with intelligence for nearly two decades, observed: "You rightly emphasize the fact that the intelligence professionals need to have more time for study and reflection. This is a great weakness of modern government everywhere. If a man is found at his desk reading a book, he is assumed to be a loafer. Actually, he may be most profitably employed. I sometimes think it would be an admirable thing if some of the top members could devote the first two hours of the day to reading and study without particular reference to any current problem."

Preparing the intelligence community for a more effective utilization of social science products is obviously a many-faceted problem for which a satisfactory solution not be discovered before a careful study has been made of recruitment and training practices and of attitudes and their manipulation by means of appropriate changes in intelligence doctrine and management. Empirical study might reveal that intelligence organizations recruit too few social scientists with a complete professional training, that mid-career staff should be given generous leaves to be spent at the universities, that staffing should be ample enough to permit wide-ranging reading, and that still other methods can be designed for transmitting relevant knowledge from the social sciences. One such method might well be the employment of social scientists for research, on a contract basis, on special intelligence problems of a kind for which they are better equipped and have more time than intelligence officers. Such use of social science

experts is practiced by many government departments, and there is no reason why intelligence should not benefit from it.

Effective utilization of social science products does not, of course, just mean _more_ use of social science materials, for these materials are of greatly varying quality and relevance, and hence must be exploited with discrimination. But discrimination should be better informed than it seems to be now and be associated with positive curiosity rather than negative skepticism.

REFERENCES

1. Almond, Gabriel A., and Coleman, James S. (eds.). The Folitics of the Developing Areas. Princeton, N.J.: Princeton University Press, 1960.

2. Aron, Raymond. "Evidence and Inference in History," in Evidence and Inference, ed. Daniel Lerner. (Pp. 19-47.) Glencoe, Ill.: Free Press of Glencoe, 1959.

3. Bergson, Abram, and Heymann, Hans, Jr. Soviet National Income and Product, 1940-1948. New York: Columbia University Press, 1954.

4. Bloch, Marc. The Historian's Craft. New York: Alfred A. Knopf, 1953.

5. Boulding, Kenneth   "The Coming of the Post-Civilized Age," University: A Princet   ...azine, No. 8 (Spring 1961), 20-24.

6. Burns, Arthur Lee. "From Balance to Deterrence," World Politics, 9 (1957), 494-529.

7. Collingwood, R. G. The Idea of History. Oxford, England: Clarendon Press, 1946.

8. Conquest, Robert. Power and Policy in the U.S.S.R. New York: St. Martin's Press, 1961.

9. Deutsch, Karl W. "Social Mobilization and Political Development," American Political Science Review. 55 (1961), 493-514.

10. Deutsch, Martin. "Evidence and Inference in Nuclear Research," in Evidence and Inference, ed. Daniel Lerner. (Pp. 96-106.) Glencoe, Ill.: Free Press of Glencoe, 1959.

11. Deutsch, Morton. "Field Theory in Social Psychology," in Handbook of Social Psychology, ed. Gardner Lindzey. (Vol. 1, Pp. 181-222.) Cambridge, Mass.: Addison-Wesley Publishing Co., 1954.

12. Economist. "Britain's Baby Boom" (editorial), The Economist. 200 No. 6153 (July 29, 1961), 437-438.

REFERENCES (Continued)

13. Evans, Allan. "Intelligence and Policy Formation," World Politics, 12 (1959), 84-91.

14. Festinger, Leon, and Katz, Daniel (eds.). Research Methods in the Behavioral Sciences. New York: Dryden Press, 1953.

15. George, Alexander L. Propaganda Analysis. Evanston, Ill.: Row, Peterson and Co., 1959. .

16. Hilsman, Roger. Strategic Intelligence and National Decisions. Glencoe, Ill.: The Free Press, 1956.

17. Joynt, Carey B., and Rescher, Nicholas. "The Problem of Uniqueness in History," History and Theory: Studies in the Philosophy of History, 1, No. 2 (1961).

18. Kendall, Willmoore. "The Function of Intelligence," World Politics, 1 (1949), 544-552.

19. Kent, Sherman. Strategic Intelligence for American World Policy. Princeton, N.J.: Princeton University Press, 1949.

20. Lasswell, Harold D. "Strategies of Inquiry: The Rational Use of Observation," in The Human Meaning of the Social Sciences, ed. Daniel Lerner. (Pp. 89-113.) New York: Meridian Books, Inc., 1959.

21. _____ . "The Policy Orientation," in The Policy Sciences: Recent Developments in Scope and Method, eds. Daniel Lerner and Harold D. Lasswell. (Pp. 3-15.) Stanford, Calif.: Stanford University Press, 1951.

22. _____ . "The Scientific Study of International Relations," The Year Book of World Affairs 1958. London: Stevans and Sons, Ltd., 1958.

23. Lazarsfeld, Paul F., and Rosenberg, Morris (eds.). The Language of Social Research. Glencoe, Ill.: The Free Press, 1955.

24. Leites, Nathan C. The Operational Code of the Politburo. (RAND report no. R-206.) Santa Monica, Calif.: RAND Corporation, 1950. (Also published by McGraw-Hill Book Co., Inc., New York, 1951.).

25. Lerner, Daniel. "On Evidence and Inference" (Introduction), in Evidence and Inference, ed. Daniel Lerner. (Pp. 7-18.) Glencoe, Ill.: Free Press of Glencoe, 1959.

26. March, James G.. and Simon, Herbert A. Organizations. New York: John Wiley & Sons, 1958.

27. Meehl, Paul E. Clinical versus Statistical Prediction. Minneapolis: University of Minnesota Press, 1954.

28. Millikan, Max F. "Inquiry and Policy: The Relation of Knowledge to Action," in The Human Meaning of the Social Sciences, ed. Daniel Lerner. (Pp. 158-1ఄ0.) New York: Meridian Books, Inc., 1959.

29. Modelski, George A. "The Communist International System" Research monograph, 9. Princeton, N.J.: Princeton University, Center of International Studies, 1960.

30. Pettee, George S. The Future of American Secret Intelligence. Washington, D.C.: Infantry Journal Press, 1946.

31. Platt, Washington. Strategic Intelligence Production: Basic Principles. New York: Frederick A. Praeger, 1957.

32. Pool, Ithiel de Sola. "Content Analysis for Intelligence Purposes," World Politics. 12 (1960), 478-485.

33. _____, and Abelson, Robert P. "The Simulmatics Project," The Public Opinion Quarterly, 25 (1961), 167-183.

34. Ransom, Harry Howe. Central Intelligence and National Security. Cambridge, Mass.: Harvard University Press, 1958.

35. Rapoport, Anatol. Fights, Games, and Debates. Ann Arbor: University of Michigan Press, 1960.

36. Reichenbach, Hans. "Probability Methods in Social Science," in The Policy Sciences: Recent Developments in Scope and Method, eds. Daniel Lerner and Harold D. Lasswell. (Pp. 121-128.) Stanford, Calif.: Stanford University Press, 1951.

37. Samuelson, Paul A. "What Economists Know." in The Human Meaning of the Social Sciences, ed. Daniel Lerner. (Pp. 183-213.) New York: Meridian Books, Inc.. 1959.

38. Sprout, Harold and Margaret. "Environmental Factors in the Study of International Politics," Journal of Conflict Resolution, 1 (1957), 309-328.

39. Wasserman, Benno. "The Failure of Intelligence Prediction," Political Studies, 8 (1960), 156-169. ·

V

INTERNAL WAR: THE PROBLEM OF ANTICIPATION

Harry Eckstein

The term "internal war" denotes any resort to violence within a political order to change its constitution, government, or policies. It is not a new concept; distinctions between external and internal war (guerre extérieure and guerre intérieure) were already made in the nineteenth century by writers on political violence.* Nor does "internal war" mean quite the same thing as certain more commonly used terms, such as revolution, civil war, revolt, rebellion, guerrilla warfare, coup d'état, terrorism, or insurrection. It denotes the genus of which the other terms are species.

The use of the general term alongside, or even in place of, the more specific terms is justifiable on many grounds, but three above all. First, and most obviously, all cases of internal war do have things in common, however much they differ in detail. All involve the use of violence to achieve purposes which can also be achieved without violence. All indicate a breakdown of some dimension in legitimate political order as well as the existence of collective frustration and aggression in a population. All presuppose certain capabilities for violence by those who make the internal war and a certain incapacity for preventing violence among those on whom it is made. All tend to scar societies deeply and to prevent the formation of consensus indefinitely. There is, consequently, at least a possibility that general theories about internal war may be discovered--general theories which may also help to solve problems posed by specific instances.

The second justification for grouping internal wars in a single universe is that actual instances of internal war often combine different types of violence, in space and time. Guerrilla warfare in one area may be combined with terrorism in another; it may be preceded by insurrections and develop into full-scale civil war, or culminate in a mere coup d'état. Indeed the large-scale and prolonged instances of internal war that we generally call revolutions are notable chiefly for the fact that they

---

Born in Germany in 1924, Harry Eckstein, PhD, served in the United States Army from 1943 to 1946 with two years in the infantry and in intelligence in the Pacific Theater. He received his PhD from Harvard in 1953 and followed this with two years postgraduate study at the University of London. Currently he is Professor of Political Science and Research Associate in the Center of International Studies at Princeton University.

---

*For example, see Kropotkin (16). The term was used by Count Fersen as early as 1790 and also occurs in the writings of Sismondi.

combine, in strikingly similar sequences, many different types of violence.* To focus analysis on particular species of internal war therefore makes it necessary to abstract from actual internal wars occurrences which may not in fact be strictly separable. This may be fine for some purposes, but it is likely to be unfortunate for most--above all, for purposes of policy. Policy-makers cannot select for action analytically distinct aspects of concrete reality and hope to achieve their ends. Neither can historians.

Finally, the terminologies presently used to distinguish types of internal war vary greatly from one another; are generally ambiguous; often define overlapping phenomena, or phenomena difficult to distinguish in practice; and are rarely based on clearly discernible analytical needs. For few phenomena do social science, history, and conventional language offer so various and vague a vocabulary. Consider a few examples--a mere sample of what there is to consider. Lasswell and Kaplan (17, pages 272-284) divide the universe of internal wars into palace revolutions, political revolutions, and social revolutions. Palace revolutions, as they define the term, are changes in rules contrary to the "political formulas" of government (that is, their formal constitutions), usually effected by members of the ruling group themselves and rarely leading to important changes in policy; political revolutions are changes in "authority structures" (formal power structures); and social revolutions are changes in the overall "control structures" (effective power structures) of society, usually effected by men not already in ruling positions.

A conventional distinction often made is that between revolutions and rebellions, the former being something broader in purpose and more tightly organized, as well as longer in duration and more violent than the latter. Gabriel Bonnet (5, pages 34 ff.), perhaps the most illustrious exponent of the French military doctrine of "revolutionary warfare," distinguishes between civil wars, wars of liberation, and revolutionary wars, with civil wars further divided into riots, insurrections, pronunciamentos, and revolutions--terms which perhaps speak sufficiently for themselves. Huntington (13, pages 22-23) talks about revolutionary wars and coups d'état, the key distinctions between them being their duration (coups are decided quickly, revolutions are not) and the extent to which the insurgents already participate in the existing system of power (they do in coups, but not in revolutionary wars). Coups are then further distinguished into governmental coups, which lead to no significant changes in social or political institutions; revolutionary coups, which do attempt to achieve such changes and reform coups, which fall somewhere between the other two. George Blankster (4, chapter 5) speaks of "real" revolutions, "near" revolutions,

---

*Brinton (7, page 1) writes: "The [French] Revolution is a series of shocks, each shock displacing power from Right to Left, from larger groups to smaller and more determined groups, each shock taking on more and more the aspect of a coup d'état, less and less that of a widespread, spontaneous outbreak of the people, until finally, in a commonplace coup d'état hardly worthy of a good operetta, power comes to rest in the hands of the dictator Bonaparte." This inspired characterization of the French Revolution might well serve as a rudimentary developmental model for any internal war that begins in large-scale, mainly spontaneous, popular violence.

and mere non-constitutional changes of government in Latin America, and Stokes (32) uses a still more complicated typology to characterize Latin American political violence: machetismo, cuartelazo, golpe de estado, and revolución.

One can derive from these and similar classificatory schemes a sort of composite typology, distinguishing between relatively unorganized and spontaneous riots by crowds with low capabilities for violence and modest aims, coups d'état by members of the elite against other members of the elite, full-scale political revolutions to achieve important constitutional changes, social revolutions to achieve large-scale socio-economic as well as constitutional changes, and wars of independence to achieve sovereignty in a previously dependent territory. But this typology is not necessarily better than the others. It does relate classes by including most of the terms of the complicated schemes and adding more terms to the simple ones; but this does not make the classification more precise, easier to apply, or more suited to actual tasks of study.

For all of these reasons it can do no harm to consider internal wars as all of a piece at the beginning of inquiry and to make distinctions only as they become necessary or advisable. In this way, the possibilities of developing general theories are increased as is the likelihood that the distinctions made will be important and precise. In any event, I shall begin here without distinctions, though it may well become necessary to introduce them before the end.

REASONS FOR STUDYING INTERNAL WARS

It ought not to be necessary to urge the study of internal wars upon anyone, social scientist or policy-maker. One would expect social scientists to be interested in them simply because they are a common, indeed astonishingly common, facet of human experience. In countries enjoying a high degree of internal peace one does tend to think of political violence as something unnatural to societies much as one thinks of illness as abnormal to the condition of individuals usually enjoying good health. But this view is culturally biased. It is certainly not justified by the actual incidence of internal wars. As indicated by the list in the Appendix to this study (see pages 141-147), societies seem to be internally at war, or on the verge of war, or engaged in healing the wounds of war about as often as they are at peace. If one can regard international war as diplomacy continued by other means, then internal war can be regarded, with far more reason, as an extension of internal politics. And just for that reason, questions about internal war--how to prevent them or instigate them, predict the courses they are likely to take and the outcomes they are likely to have, win them or defeat them, interfere in them or isolate them, restore tranquility after them--ought to be matters of the most urgent concern to policy-makers, even in the most tranquil countries.

The importance of studying internal wars would seem not to need underlining; it is surely obvious. Nevertheless, there are some important reasons for agitating in behalf of studies of internal war.

One is that modern social scientists have in fact shamefully neglected the subject. An indication of this neglect is the terminological muddle

described above. Another is that a recently published mammoth compendium of social thought (26), which runs to some 1,400 pages and contains 168 selections from leading social thinkers and scientists, makes hardly a reference to the phenomenon of political violence.* Still more indications to the same effect will be found abundantly throughout this paper. I shall refer to the work of historians, revolutionaries, and obsolete sociologists far more frequently than to works by contemporary systematic social scientists.

One reason for this may be that contemporary social science has been predicated far too much upon perspectives which regard violence, a la Hobbes, as the very negation of the social condition; approach social analysis in a predominantly static manner; and place heavy emphasis on the ideas of equilibrium, integration, and systematic inter-relation. In addition, a great deal of modern social science in the Western World has developed in conscious reactions against social theories which were deeply preoccupied with political violence: for example, dialectical materialism, certain social evolutionary theories which found in violence a mainspring of social development, and the violence-loving social romanticism of syndicalists and fascists. So large does violence loom in these theories, discredited and rejected in the Western World, that modern social scientists seem to feel impelled to ignore the phenomenon itself, no less than the theories which emphasize it. This is not to say that modern social science cannot provide useful insights into internal war but only that modern social scientists have uniformly failed to provide them.

There is another reason for making a special plea on behalf of the study of internal wars at this time. However common-place internal wars have been in the past, we have reason to think that they will remain commonplace in the future--and even more intimately connected with policy. Most important, while internal wars have always been extensions of domestic politics, there are good reasons to believe that they will become especially important in our age as extensions of international politics.

Much is in doubt about the causes of internal war, as we shall see, but of one proposition we may be certain: internal war is closely connected with social change. Sometimes such war is a product of the tensions attendant upon social change, sometimes a method for bringing change about, or sometimes a means of overcoming rigidities in social structure. This interrelationship is hardly a very penetrating discovery. However, from this basic assumption it follows that one should expect internal political violence to persist, perhaps to increase, in a period when major social change continues to be so widespread and so widely wanted. The tremendous number of internal wars in the period 1946-1959 (see Appendix)--and in other "transitional" periods, such as late antiquity, the Renaissance and Reformation, and the early nineteenth century in Europe--is evidence of this.

Moreover, the likelihood of internal war seems to be increased at least in some degree by the shrinking probabilities of international war in

---

*Three very brief selections from the works of Pareto, Durkheim, and Simmel are about all in this enormous volume that can be considered directly relevant to the subject.

the age of "overkill." Huntington (13, pages 17-19), among others, has ob·
served that there has never been another period in history so afflicted with
international tension and yet so free of international war as our own. This
fact surely is due in large part to the risks involved in contemporary war-
fare, but it also arises, perhaps, from certain other, less obvious, factors:
modern communications, which make it less possible to confine and local-
ize wars; the moral climate now existing in international politics; and the
fact that, despite great international tensions, war is no longer considered
one of the normal, almost routine, affairs of state and a necessary adjunct
to diplomacy, as it was in the heyday of the power state.

From the mere fact that international war is now rather less likely
than in the past it does not, of course, follow automatically that internal
war must be more likely. But one can arrive at this conclusion without
applying to political violence some analogue of the mercantilist conception
of wealth. Whatever the likelihood of international war, nation-states still
have interests, or conceptions of interests, some of which may appear to
them to warrant the use of egregious means. And one of these means may
well be the instigation or support of internal wars, wherever the possibility
to do so offers itself. Many internal wars are extensions of diplomacy to-
day, and it is clear that in the future they may become even more signifi-
cant as "diplomatic" means.

The opportunities for internal wars have grown along with their at-
tractions to the world powers--a lethal combination. Officers of the French
army have already developed, on the basis of this point of view, a whole
military-political doctrine for the nuclear age, the theory of guerre révo-
lutionnaire, just as some Americans have been busy spinning theories of
"indirect aggression"--the stimulation of existing discontent into violence
by the provision of arms and bases, and by propagandistic incitement,
whether in favor of communism or its opponents (13, page 26).

Indeed, one reason it makes sense to speak of internal "wars" is pre-
cisely the fact that violence within a state--and violence of many kinds--
can be used for the same purposes as violence between states. We could
in fact define internal war as war conducted by any means other than direct
and overt conflict between states, and such a definition would have one im-
portant virtue: it would direct attention immediately to the international
aspects of internal war without necessarily blinding the observer to the
domestic forces involved in it.

It is particularly likely today that the communist states will use in-
ternal wars as tools of international politics. They have already done so
frequently in the postwar period and are likely to do so frequently in the
future. Communism--especially the present-day Chinese version--is a
militant messianic creed, and such creeds always tend to produce expan-
sionist zealots. If the use of conventional warfare is closed to the com-
munists as a means of exporting their utopia they will try other means to
the same end, and the means most readily available is the instigation and
clandestine support of internal violence. Furthermore, communism places
no immediate normative prohibitions on political violence; on the contrary,
communism justifies and glorifies violence. In communist doctrines, of
course, violence is purely instrumental to other ends; among communists,
however, violence may well be valued in itself. Certainly it is no accident
that so many left extremists have so easily made the switch to fascism--

Mussolini himself is an example. Furthermore, communists have avail-
able in other countries much material for fashioning internal wars: much
desperate discontent, many sublime and frustrated hopes, and much ana-
chronistic hatred of non-communist Western systems. Most important of
all, the communists have an enormous lead in the experience of and reflec-
tion upon internal war. If we are far ahead of them in deterrence theory,
they are immeasurably farther ahead of us in revolutionary theory. They
have been in the business for a long time and, what is more, have thought
long and deeply about it. Much of what we know about guerrilla warfare,
for example, we have learned from them, and it is unlikely that they have
told us all they know.

The Communists must realize that in practically any internal war they
have crucial advantages over their opponents: lack of scruples. a messi-
anic vision, much potential local support, and a closely reasoned doctrine
of revolutionary action based, in part, on extensive and successful trial
and error. Why then should they not be perfectly serious in their protes-
tations against regular international warfare and still be serious about
their expansionist designs?

No doubt some such view underlies the recent, still rather tentative,
American attempts to understand guerrilla, or irregular, warfare and to
train men for it. This development comes to grips with at least one of the
problems of internal war and is of interest to social scientists no less than
policy-makers. But the preoccupation with guerrilla warfare also involves
a danger: namely; that the quest for operational rules of prudence will
take the place of more general inquiry into internal wars. This would be
extremely unfortunate, not only for the social scientist, but for the policy-
makers as well. From a practical standpoint, we need a general study of
internal war even more than a study of the operational aspects of irregu-
lar warfare. The main reasons are that in most conceivable future inter-
nal wars it is we who are likely to be on the defensive and because in no
other field is prevention so much more effective than cure. However im-
portant operational maxims may be to the rebellious (the insurgents), the
fate of the incumbents turns mainly on the early detection of internal war
dangers by means short of revolutionary violence.

At present, the most probable kinds of internal war, once started, are
difficult, if not impossible, to win by those on the defensive. Above all is
this likely to be true of guerrilla fighting in favorable terrain (and almost
any terrain is favorable if used correctly by guerrillas), if the noncombat-
ant population is well-disposed toward the guerrillas and the incumbents
have any sort of scruples. Under such conditions it is difficult even for
unsophisticated guerrillas to make fatal mistakes. The troubles of the
French in their overseas territories furnish excellent examples; so does
Cuba; so even does Malaya, where a superiority of 25 to 1 in manpower
and ten years of fighting were needed to put down guerrilla fighters who
did not by any means command undivided popular support. The operational
lore of revolutionary warfare is short and simple perhaps just because
there may not be a great deal to say on the subject if conditions rather
than strategy and tactics are all-important.

Insofar as strategy and tactics matter, they seem to be useful almost
exclusively to the rebels, not to established authorities providing routine
government in extensive territories. Certainly it is ludicrous to think that

guerrilla warfare can be used to cope with guerrilla warfare and to as-
sume that what works for one side is likely also to work against it. Es-
tablished governments cannot melt away into hills and cellars, or rely
upon clandestine methods of supply, or surrender routine administration
and the regular army to the rebels in order to make irregular warfare
upon them. Defending against guerrillas is not onl, different from carry-
ing out rebellious guerrilla operations but also far more difficult. One
need only look at guerrilla and counter-guerrilla doctrines to realize this.
Guerrilla doctrines seem to be remarkably widely agreed upon, and suc-
cessful. Yet years of anguished study by counter-guerrilla officers, like
those pathetic exponents of guerre révolutionnaire, seem to have produced
only an abundance of conflicting views and practices--all apparently futile,
and some dangerous to boot.

Not only is cure unlikely once guerrilla fighting has occurred under
favorable conditions, it is also immensely costly if it is to have any chance
of success. A tremendous preponderance of men and resources is re-
quired by the defensive side--no one really knows how much. The fighting
is likely to be prolonged and vicious. And physical costs are not the only
costs exacted in guerrilla warfare. One must pay great moral and psych-
ological costs as well. For example, we know that the central problem
in defense against guerrilla warfare is intelligence: knowing what an in-
finitely elusive enemy is really up to. We also know that against compe-
tent guerrillas and their enthusiastic civilian supporters, adequate intelli-
gence seems to require brutal methods such as torture and terroristic
intimidation (especially if anti-guerrilla civilians are liable to suffer
guerrilla retaliation). The stakes are too high and the chances of failure
too great for pretty manners to be observed. Guerrilla warfare thus
breeds savagery even in the best-intentioned of defenders. It makes them
appear corrupt and it corrupts them. Guerrilla warfare, used in defense
of the state, is likely to breed new dangers to the state. That is the moral
cost. The psychological costs arise from the need to live with a kind of
warfare more uncertain, more elusive, and more unlikely to be fully re-
solved than any kind of conventional warfare.

Even if these costs are borne and the internal war suppressed, the
matter is not necessarily ended. Internal wars, in almost any form, tend
to scar and unsettle societies for indefinite periods, no matter who wins
them and no matter what their objects. That much at least we know about
the "price of revolution." Putting down a revolution is never quite the
same as preventing its occurrence; promoting revolution never quite the
same as achieving one's objectives by other means.

The very fact of internal war will have long-range repercussions on a
society (and thereby possibly on international politics) even if the members
of society are predominantly in favor of the side that wins. Certainly the
repercussions of internal war run much deeper and much longer than those
of international war. History suggests that the wounds and animosities of
international war are quickly forgotten, if not healed; and history suggests
that those of civil strife are only rarely overcome, short of the longest of
long runs.

Some societies, indeed, seem never to overcome the effects of civil
strife at all. One may doubt, for example, that France has ever gotten
over the effects of the French Revolution and, on even better grounds, that

truly legitimate authority has ever become established in much of Latin America after the first age of revolution. At the very least, internal wars differ from international wars in one crucial respect: they are not necessarily won when the enemy has been defeated. There remains the problem of restoring legitimate order. The problem, in essence, is that of making loyal citizens out of defeated rebels. Merely defeating the rebels is not likely to suffice for that; nothing may suffice.

In a world of internal wars it is certainly necessary to analyze and comprehend the operations of internal war. But to concentrate only on military operational questions is the next worst thing to not studying internal wars at all. The most important problems internal wars raise are precisely those we now study least: (a) how to anticipate internal wars (that is, how to discover the preconditions of internal war), (b) how to prevent them, and (c) what to do after hostilities cease. Of these problems, the first two are obviously the more important. If they are solved, the third need never arise.

All this gives added point to the frequently repeated statement that internal war adds a new dimension, a political dimension, to the problems of warfare. Internal war is a struggle for political loyalties no less than military victories, a struggle requiring intense political consciousness on both sides. Indeed, the political art of detecting internal war potential must have priority over the military art of fighting it. This applies to military policy-makers no less than civilian ones. In a world of alliances, foreign bases, and far-flung power blocs, detecting in advance the instability of regimes and knowing how to shore them up with fair chances of success are among the most urgent imperatives of the military as well as the political arts.

That is why the rest of this paper is devoted to the problems involved in anticipating internal wars. No one, as yet, can solve these problems. But I can convey some idea of available thought upon them, the difficulties they pose, the analytical choices they require one to make, and the more profitable lines that can be pursued in the search for solutions. Many of the ideas and data presented concern events long past rather than contemporary cases, but that is a reflection of the long dormant concern with the problems of internal war, both in social science and public affairs. In any event, even if many of the ideas and data discussed below have a musty historical flavor, they have, in every case, considerable relevance to current events.

CAUSES OF INTERNAL WAR

To anticipate internal wars correctly one needs, first of all (but not only), an understanding of the forces that impel societies toward them -- that is, their causes. In regard to this subject, the literature of social science and history presents us with one problem above all: how to choose among a rare abundance of hypotheses which cannot all be equally valid. This problem results not from too much study of internal wars but from too little, and the wrong kind. It exists because most propositions about the causes of internal wars have been developed in historical studies of particular cases rather than in genuinely social-scientific studies. In historical case studies one is likely to attach significance to any aspect of

pre-revolutionary society one intuits to be significant, and so long as one
does not conjure up data out of nothing, one's hypotheses cannot be invali-
dated on the basis of the case in question.

That most studied of all internal wars, the French Revolution, pro-
vides a case in point--as well as examples in abundance of the many so-
cial forces to which the occurrence of internal wars might be attributed.
Scarcely anything in the French ancien régime has not been blamed, by
one writer or another, for the revolution. And all of their interpretations,
however contradictory, are based on solid facts.

Some interpreters have blamed its outbreak on intellectual causes,
that is to say, on the ideas, techniques, and great public influence of the
philosophes (who were indeed very influential). This is the standard the-
ory of post-revolutionary conservative theorists, from Chateaubriand to
Taine, men who felt, in essence, that in pre-revolutionary France a sound
society was corrupted by a seductive and corrosive philosophy.

Other writers have blamed the revolution on economic conditions, al-
though it is difficult to find very many who single out as crucial the same
conditions. The revolution has been attributed (a) to sheer, grinding pov-
erty among the lower classes (who were certainly poor); (b) to financial
profligacy and mismanagement on the part of the government (of which it
was in fact guilty); (c) to the extortionate taxation inflicted on the peasants
(and peasant taxation verged upon brutality); (d) to short-term setbacks
(which actually occurred and caused great hardship) like the bad harvest
of 1788, the hard winter of 1788-89, and the still winds of 1789 which pre-
vented flour from being milled and made worse an already acute shortage
of bread; (e) to the overabundant wine harvests of the 1780's (one of the
first historic instances of the harmful effects of overproduction); (f) to the
increased wealth and power of the bourgeoisie in a society still dominated
to a significant extent by aristocrats; (g) to the growth of the Parisian pro-
letariat and its supposedly increasing political consciousness, and (h) to
the threatened abrogation of the financial privileges of the aristocracy,
particularly their exemption from taxation. These unquestionable facts all
produced manifest problems.

Still another set of writers locates the crucial cause of the revolution
in aspects of social structure. Much has been made, and with sufficient
reason, of the fact that in the last years of the ancien régime there oc-
curred a hardening in the lines of upward mobility in French society. For
example, there was a decline in grants of patents of nobility to commoners,
and stringent social requirements were imposed for certain judicial and
administrative positions and for purchased officerships in the army. These
conditions, many have argued (following Pareto's (25) famous theory of the
circulation of elites), engendered that fatal yearning for an aristocracy of
wealth and talent to which the philosophes gave expression.

Much has also been made, with equal reason, of popular dissatisfac-
tions with the parasitic life of the higher nobility, with its large pensions
and puny duties; its life of hunting, love-making, and watch-making; and
its interminable conversation. And much has been attributed to the vul-
nerability of these privileged classes to the very propagandists who wanted
to alter the system that supported them. "How," asked Taine (33), "could

people who talked so much resist people who talked so well?" Their vulnerability was reflected in the Anglomania which swept through the higher aristocracy toward the end of the <u>ancien régime</u> and in the rush of many aristocrats to the cause of the American colonials in their war of independence.

There are also certain well-founded "political" explanations of the French Revolution. Among these are the following two: (a) that the revolution was really caused by the violation of the tacit "contract" on which the powers of the monarchy rested (a contract by which the aristocracy surrendered its powers to the monarchy in return for receiving certain inviolable privileges) and (b) that the revolution was simply a successful political conspiracy by the Jacobins, based on an efficient political organization. Personalities, needless to say, get their due as well. The revolution has been blamed, for example, on the character, or lack of character, of Louis XVI (who was in fact weak, vacillating, and inconsistent); the supposed immorality of the Queen (who indeed was the subject, justly or not, of many scandals); the effect on the public of the dismissal of Necker; and, of course, on the "genius," good or evil, of unquestionable geniuses like Mirabeau, Danton, Marat, and Robespierre.

We could take other internal wars and arrive at the same results—similarly large lists of explanations, most of them factual, yet inconclusive. The more remote in time, and the more intensively analyzed, the internal war, the longer the list of hypotheses one would expect. Yet even so recent a case as the Chinese Communist Revolution has given rise to a fearful number of plausible hypotheses, many directly contradictory.

The Chinese Revolution has been blamed on plain conspiracy: a plot by a small number of Kremlin agents or power-hungry Chinese intellectuals. It has been blamed on social forces, like the rise of an urban working class or a "new" urban middle class, large-scale mobility from countryside to cities and the attendant weakening of traditional social patterns (the growth of a "mass" society*), or the effects of a "population explosion." Its genesis has been located in political culture, structure, and decisions; in hyper-nationalism and the hatred it engendered of those cooperating with "imperialist" powers; in the corruption of the incumbent regime; in its suppose- / selfish and reactionary policies, ignoring the interests of land-hungry peasants, urban workers, and the new urban middle class; in administrative incompetence; in exhorbitantly harsh policies toward all actual and potential opposition.

Economic explanations are available too. The revolution, it has been argued, was caused by general poverty, by the exploitation particularly of the peasants, by the breakdown of the peasant crafts due to Western imports, by intellectual unemployment, and by various natural disasters. Finally, there are the usual theories about great and puny men. Not least, there is the interpretation perhaps most familiar to us of all: that the

---

*For an explanation of this term and its use in the explanation of large, illegitimate political movements, see Kornhauser (15).

revolution was caused by insufficient and untimely external support for the incumbents compared to the external support available to the insurgents.*

What is needed to transform this chaos into order is systematic comparative study. The examination of any single case allows one to determine only whether an explanation is based on fact; comparative study is needed to determine the significance of the facts, wherever this cannot be established by the application of already validated general theories. But even prior to such studies, some order can be introduced into the subject. We can at least classify the explanations available, indicate the analytical choices they require and do not require, and (as an indispensable first step toward coming to grips with the problem) attempt to determine what lines of analysis are most likely to prove rewarding for both social scientists and policymakers. This is what I propose to do in the following section.

"Precipitants" and "Preconditions" of Internal War

Perhaps the first thing that becomes apparent when one tries to classify causal explanations of the sort sketched above is that many of the explanations do not really pose a "necessity for choice" on the part of the analyst. The prepositions do not always contradict one another; often, in fact, they are complementary, differing only because they refer to different points in the time-sequence leading to revolution   because they refer to different kinds of causality, or because they s   out one factor among many of significance.

The most important distinction to make in this connection is between precipitants and preconditions of internal wars. A "precipitant" of internal war is an event which actually starts the war (which "occasions" it), much as turning the flintwheel of a cigarette lighter ignites a flame. "Preconditions" of internal war, on the other hand, are those circumstances which make it possible for the precipitants to bring about political violence, just as the general structure of a lighter makes it possible to produce a flame by turning the flintwheel. Some of the causal explanations of the French Revolution clearly fall into the "preconditions" category (e.g., the conditions leading to severe bread shortages in Paris shortly before the convening of the Estates-General), while others fall equally clearly into the "precipitants" category; and between explanations singling out precipitants and explanations emphasizing preconditions of internal war there obviously is no genuine contradiction. The distinction between precipitants and preconditions can therefore prevent much pointless argument between those who stress short-run setbacks and those who emphasize long-term trends in the etiology of civil strife. Clearly no internal war can occur without precipitant events to set it off; and clearly no precipitants can set off internal war unless the condition of society makes it possible for them to do so.

---

*I am indebted to my colleague David B. Bobrow for this list; his, I should mention, was in fact much longer.

For a similar, still more extensive, treatment of the literature on the American Civil War, which is also very instructive on the general problems of causation of historical events, see Benson and Strout (3). See also Pressly (28).

The greatest service that the distinction between precipitants and pre-conditions of internal war can render, however, is to shift attention from aspects of internal war which defy analysis to those which are amenable to systematic inquiry. Phenomena which precipitate internal war are almost always unique and ephemeral in character. A bad harvest, a stupid or careless ruler, moral indiscretion in high places, an ill-advised policy: how could such data be incorporated into general theories? These conditions are results of the vagaries of personality, of forces external to the determinate interrelations of society, of all those unique and fortuitous aspects of concrete life which are the despair of social scientists and the meat and drink of narrative historians.

But if one fails to take note of such conditions and focuses solely on data the social scientist can handle, does one not leave out of consideration a great deal that really matters to policy-makers in the study of internal wars? On the contrary: preconditions should be the crucial concern of policy-makers no less than social scientists. After all, the object of policy-makers in studying the etiology of internal wars is to anticipate such wars in good time in order to prevent them when they are preventable, to further their actual occurrence, or otherwise to prepare for them. But unique and ephemeral phenomena cannot, in their very nature, be anticipated. They simply happen, and having happened cannot be undone. The vital knowledge needed concerns those conditions under which almost any setback or vagary, any unguided policy or indiscretion, can set society aflame.

Certain kinds of precipitants of internal war have a special importance of their own, however. A precipitant may be found so frequently on the eve of internal wars that its existence can be treated as an urgent danger signal, particularly if its effects are delayed sufficiently to allow sensible adaptation to the danger.* As far as we know, both of these conditions are satisfied by economic precipitants of internal war. The point deserves some elaboration, particularly in view of the persistent emphasis on economic conditions in writings on internal war.

It now seems generally agreed that persistent poverty in a society rarely leads to political violence. Quite the contrary. As Edwards (10, page 33) points out, following an argument already developed by de Tocqueville (34), economic oppression, indeed all kinds of oppression, seems to wane rather than increase in pre-revolutionary periods. Brinton (6, pages 29-37) makes the same point. While not understanding the amount of poverty in the societies he analyzes in The Anatomy of Revolution, he does point out that all of these societies were economically progressive rather than retrograde. He points out also that revolutionary literature, at any rate in the pre-Marxist period, hardly ever dwelt on economic misery and exploitation; one hears about economic grievances, to be sure, but not the sort of grievances which arise out of "immiseration." Even some Marxists seem to share this view. Trotsky, for example, once remarked that

---

*In that case, of course, one could quibble about whether it really is a precipitant. Suffice it to say that I use the term "precipitant" precisely because I want to avoid the connotation of the more familiar concept, "immediate" cause.

if poverty and oppression were a precipitant of revolution the lower
classes would always be in revolt, and obviously he had a point.

It is equally difficult to establish a close link between economic im-
provement and internal war. Prerevolutionary periods may often be eco-
nomically progressive, but economic progress is not always (or even often)
connected with internal war. From this, however, one need not conclude
that economic tendencies are simply irrelevant to the occurrence of politi-
cal violence. Only the long-term tendencies seem, in fact, to be irrelevant.
The moment one focuses on short-term tendencies, a fairly frequently re-
peated pattern emerges--and one which tells us why it is that some writers
adhere stubbornly to the immiseration theory of internal war and others,
with just as much conviction, to the economic progress theory. It so hap-
pens that before many internal wars, one finds both economic improvement
and immiseration; more precisely, many internal wars are preceded by
long-term improvements followed by serious short-term setbacks.* The
bad harvests and unfavorable weather conditions in prerevolutionary
France, the American recession of 1774-1775, the bad Russian winter of
1916-1917 (not to mention the economic impact on Russia of the war), and
the marked rise of unemployment in Egypt before Naguib's coup are cases
in point. All dealt serious short-term blows to economic life, and all fol-
lowed long periods of economic progress, especially for those previously ·
"repressed."

It is this dual pattern which really seems to be lethal; and it is not
difficult to see why. In times of prolonged and marked economic progress,
people become accustomed to new economic standards and form new eco-
nomic expectations which previously they could scarcely imagine. Confi-
dently expecting continuous progress, they also tend to take certain risks
(like accumulating debts) which they might not take herwise. All this
greatly exaggerates the impact of serious temporar, setbacks. Both psy-
chologically and economically the costs of such setbacks are bound to be
greater than if they occurred after long periods of stagnation or very
gradual progress.

Conspiracy Theory

Being concerned mainly with the precipitants of internal war is likely
to have one especially regrettable consequence: it may lead to an unquali-
fied conspiracy theory of internal war. In every internal war of any con-
sequence we do find conspiratorial organizations, either before the out-
break of such wars or not long after--in one case, Jacobins; in another,
fascists; in still another, communists. A temptation exists, in conse-
quence, to attribute the internal war solely or mainly to the conspirators.
to regard it mainly as a product of technique--Machiavellian plotting on
one hand and insufficient intelligence or suppression on the other. The
literature on internal war abounds in such interpretations; not least, that

---

*See Davis (8). This paper traces the pattern in Dorr's Rebellion.
the Russian Revolution, and the Egyptian Revolution.

dealing with contemporary internal wars.* Not only do we tend to see communist conspiracies behind all contemporary internal wars, but we also seem often to assume that without communist conspiracies there would be no difficulties in the societies concerned. The first view could well be true; but the second is fallacious, and there is much evidence to suggest that it is indeed fallacious. As often as not, communists have seized upon internal war situations rather than creating them. They did so in the Russian Revolution itself, in China, and, as far as we know, in Cuba. Certainly in the first two cases the forces behind the outbreak of violence were rather inchoate social forces, from which the conspirators profited but which they did not bring about. In the third case, the forces leading to internal war were partly inchoate and partly organized, but despite Castro's belated profession of Marxist-Leninist faith, probably not communist. The communists just managed successfully to mobilize these forces for their own ends, possibly not until after the actual fighting had ended. If internal wars consisted solely of quick and simple coups d'état, or palace revolutions, it might be worthwhile bothering with the conspiracy theory of revolution. But something more than unchecked plotting is needed to wage internal war on a more comprehensive scale. Plots can be little more than precipitants in such cases, but when internal war has already been precipitated, plots are more likely to become significant.

The worst consequence of the conspiracy theory of internal war is that it tends to produce irrelevant, and possibly disastrous, policy responses to both potential and actual internal war situations. From plot theory one is bound to deduce that the suppression of conspiracies, real or imagined, is enough (and alone enough) to make internal war unlikely. But action so predicated may well be self-defeating, by mistaking the real forces impelling societies toward violence and possibly adding to them. At present, for example, it tends to make us infer that any buttressing of anti-communist regimes lessens the chances of a communist takeover; in fact, however, it may increase these chances--if the policies concerned heighten a society's internal war potential, thus helping to produce situations from which conspirators may profit.

As with prerevolutionary conditions, so with revolutions themselves. Conspiracy theory compels one to assume that every internal war has an aim, an organization, a shape and tendency from the outset, and, hence, in a world in which the most likely conspirators are one's enemies, that it must be suppressed. But internal wars in their early stages may be characterized by nothing so much as amorphousness--formless matter waiting to be shaped. The communists, precisely because they are conspirators, know very well how to capitalize on such unallocated political resources. Others seem often to assume that only the status quo can serve their ends, thus forfeiting to their enemies the use of one of the most elemental and galvanic forces in social life.

---

*For a good specimen of the "plot" theory of internal war, see Webster (36). For a still better specimen, very much in the tradition of Machiavelli, see Malaparte (20). Malaparte seems to argue in effect that successful revolutions can be made in any society (he quotes approvingly Pope Pius XI's dictum to this effect), provided only that the revolutionary possess the right recipe for making them.

Knowing how to shape and channel these forces is one of the more in-
dispensable arts of government in periods of endemic civil strife. From
this standpoint, a due appreciation of the techniques of conspiracy is indis-
pensable. It is necessary to detect and forestall the designs of others and
equally necessary to direct revolutionary momentum into favorable direc-
tions. But conspiracy theory has little place in an etiology of internal wars
in general. This is not, of course, to say that conspiracy never causes in-
ternal wars. It obviously plays a crucial role in coups (palace revolutions)
and in sharply localized insurrections. In larger-scale outbreaks of politi-
cal violence, however, the role of conspiracy is mainly what Brinton (7)
implies it was in the French Revolution: to shape violence and finally re-
allocate power--in a sense, to turn revolution increasingly into coup d'état
or civil war, the types of internal war which most display design and or-
ganization.

Common Hypotheses About the Preconditions of Internal War

We can legitimately, and even profitably, ignore most of those greatly
varying and largely fortuitous events which occasion the outbreak of inter-
nal wars. But even if we do, a great variety of plausible hypotheses re-
mains--great enough if we confine ourselves to general treatments of in-
ternal war and greater still if we deal with hypotheses formulated to deal
with particular cases. In this connection, it might be illuminating to sup-
plement the lists of explanations of particular revolutions above with a
sample of propositions frequently found in the more general literature on
internal war.* These include:

(a) Hypotheses emphasizing "intellectual" factors--

      internal wars result from the failure of a regime to perform ade-
      quately the function of political socialization; internal wars are
      due to the coexistence in a society of conflicting social "myths";
      internal wars result from the existence in a society of unrealiz-
      able or corrosive social philosophies.

(b) Hypotheses emphasizing economic factors--

      internal wars are generated by growing poverty; internal wars
      result from rapid economic progress; internal wars are due to
      severe imbalances between the production and distribution of
      goods.

(c) Hypotheses emphasizing aspects of social structure--

      internal wars are due to the inadequate circulation of elites (that
      is, inadequate recruitment into the elite of the able and powerful
      members of the non-elite), internal wars result from too much
      recruitment of members of the non-elite into the elite, breaking
      down the internal cohesion of the elite; internal war is a reflec-
      tion of anomie resulting from great social mobility; internal war
      is a reflection of frustration arising from little social mobility.

_____

*These hypotheses come from a large variety of sources, including
works by Lasswell and Kaplan (17), Edwards (10), Pettee (27), Brinton (6),
Rudé (29), Trotsky (35), DeGrazia (9), Mosca (23), and Pareto (25).

(d) Hypotheses emphasizing political factors--

internal wars are due to the alienation of rulers from the societies they rule; internal war is simply a response to bad government (government which performs inadequately the function of goal-attainment); internal wars are due to divisions among the governing classes, not to the attacks of the governed on those who govern; internal wars are responses to oppressive government; internal wars are due to excessive toleration of alienated groups.

(e) Hypotheses emphasizing no particular aspect of societies, but general characteristics of social process--

political violence is generated by rapid social change; political violence results from erratic rates of social change, rather than from changes which are even in tempo, whether rapid or not; internal war occurs whenever a state is somehow "out of adjustment" to society.

From this sample of propositions, all of them plausible, we can get some idea of the overpowering ambiguities facing policy-makers who need to cope with potential internal war situations, real or imaginary, in foreign countries. Should the policy-makers pour economic aid into the country concerned? That might only accelerate economic processes which already tend to bring about internal war. Should they force political reforms upon the country? These, however beneficial in principle, might give alienated groups too much scope, or decrease the rapport between rulers and their subjects, or make government inefficient by demanding action patterns for which the governing personnel is not equipped. The very fact of interference might heighten the chances of internal war; so might the lack of it.

Similar problems confront regimes that feel threatened from within. Should they be tolerant toward enemies--try to kill by kindness, as von Papen did in the case of Hitler? Or should they try suppression and thus heighten the enmity of disaffected groups? Should they admit a great deal of new blood into their higher councils, and thus take a chance on conspiracy in high places; or should they conduct rigid purges of any dubious elements in government, and thus possibly increase the leadership resources and popular support of their enemies? Is it better to increase the rate of economic development or to slow it down, better for rulers to copy foreign ways or adhere stringently to local traditions, better to encourage grandiose hopes or to play them down, better to be frugal or spendthrift in national budgeting?

Given these ambiguities and dilemmas, it is difficult to see that anything more than an intuitive, possibly self-defeating response to internal war situations--anything more than the most tentative groping in the dark-- is possible at present. Unfortunately, however, internal wars affecting vital interests will not wait upon an exhaustive, systematic consideration of all the possibilities.

Hence the intellectual question which needs settling at this stage, above all, is whether certain lines of analysis are more likely to yield reasonably quick and useful results than others. What basic choices, other than ignoring precipitant events, should we make in trying to reduce our

ignorance about the forces that impel societies toward internal war? I am here concerned, needless to say, with <u>inquiry</u> into internal war not internal war itself, although research choices presuppose some substantive knowledge, as we shall see.

Insurgents or Incumbents?

One crucial choice that needs to be made is whether to put emphasis upon characteristics of the insurgents or incumbents, upon the side that rebels or the side that is rebelled against. Not surprisingly, the existing literature concentrates very largely on the rebels, treating internal war as due mainly to changes in the non-elite strata of society to which no adequate adjustment is made by the elite. This would seem to be only natural; after all, it is the rebels who rebel. At least some of the literature suggests, however, that characteristics of the incumbents must be considered jointly with characteristics of the insurgents, perhaps even emphasized more strongly. Pareto (25, passim), for example, while attributing revolution partly to blockages in a society's social mobility patterns, considered it equally necessary that certain internal changes should occur in an elite if revolution was to be possible; in essence, he felt that no elite which had preserved its capacity for timely and effective violence, or for effective manipulation, could be successfully assailed, or perhaps assailed at all. One must, according to this point of view, seek the origins of internal war not only in a gain of strength by the non-elite, but also in the loss of it on the part of the elite. And Brinton (6) makes the same point: revolutions, in his view, follow the loss of common values, of internal cohesion, of an unquestioned sense of destiny and superiority in elites, and thus must be considered results as much as causes of their disintegration.

Significantly enough, this view is stated perhaps more often in the writings of actual revolutionaries than in those of students of revolution. Trotsky (35, page 311), for example, believed that revolution requires three elements: (a) the political consciousness of a revolutionary class, (b) the discontent of the "intermediate layers" of society, and (c) just as important, a ruling class which has "lost faith in itself," which is torn by the conflicts of groups and cliques, which has lost its capacity for practical action and rests its hopes in "miracles or miracle workers."

The joint consideration of insurgent and incumbent patterns would seem to be the logical way to proceed at the beginning of inquiry into the causes of revolution. But one should not overlook the possibility that sufficient explanations of the occurrence of many internal wars might be found in elite characteristics alone. A ruling elite may decay, may become torn by severe conflict, may be reluctant to use power, or may come to lack vital political skills--and thus make it perfectly possible for a relatively weak, even disorganized, opposition to rise against it and destroy it. Indeed, there are theories which maintain that internal wars are always caused solely or primarily by changes in elite characteristics and that one can practically ignore the insurgents in attempting to account for the occurence of internal wars.

One such theory is propounded in Mosca's <u>The Ruling Class</u> (23). If the elementary needs of human life are satisfied, argued Mosca, only one thing can cause men to rebel against their rulers, and that is their feeling that the rulers live in a totally different environment, that the rulers are

"separated" from their subjects in some profound se .e. In his view, only
the alienation of the elite from the non-elite (not the reverse) can lead to
violent attacks upon an established order; only the elite itself, consequently,
can undermine its political position. In this regard Mosca made much of
the feudal societies of Poland, Ireland, England, and Russia. De Grazia
(9, pages 74-75) points out that the Polish nobles of the Middle Ages, for
example, practiced extreme economic extortion, taking almost all the
peasant produced in levies; they were ruthless and violent; they scrupu-
lously extracted the droit du seigneur; and despite all that, and more, the
peasants never rebelled as long as the nobles "lived among them, spoke
their language, swore the same oaths, ate the same food, wore the same
style of clothes, exhibited the same manners or lack of them, had the same
rustic superstitions." But a drastic change occurred when the nobility ac-
quired French manners and tastes and "gave luxurious balls after the same
manner of Versailles and tried to dance the minuet." Despite more hu-
mane treatment, vicious and frequent revolts attended the estrangement of
the nobles from their people.

This interpretation certainly makes sense in light of French experi-
ence: the French Revolution was far more an attack upon the refined and
parasitic court nobility than upon the coarse, and little less parasitic, pro-
vincial nobility. It makes sense also in the case of Britain, for the British
nobility (in the main) always preserved close ties to the soil and to the
manner and morals of its tenantry. That is why Britain was for so long
the butt of jokes among the more sophisticated, and shorter-lived, conti-
nental aristocracies.

Perhaps the most prolonged period of civil unrest in American his-
tory, the late nineteenth century, can be, and has been, interpreted in much
the same manner--not only by political sociologists like De Grazia (9, es-
pecially pages 117 ff.) but also by acute literary observers like Mark
Twain and historians like Miriam Beard (2). One of the more glaring fea-
tures of that period was the compulsive attempt of the American monied
aristocracy to imitate European "society." At no other time in American
history was the elite so profoundly estranged from American life. Miriam
Beard (2, pages 643-644) has written:

> "The last two decades of the nineteenth century offered a spectacle of
> parvenudom without a peer. Ostentations display was given the utmost
> press publicity, for a man like 'Silver Dollar' Tabor was only too anx-
> ious to have the world know that even the door-knobs in his palace
> were of silver. No more abashed in their magnificance than royalty
> of the European baroque, were the American business men, who had,
> like kings, made a nation; but the splendor of kings had seemed, not
> personal, but an integral aspect of their office .... It was this differ-
> ence which caused the naive whims of the new-rich to exercise so fatal
> an effect on the public mind of America .... In Waldorf's 'Peacock
> Valley' and in San Francisco's 'Poodle Dog,' millionaires paraded in
> their newest finery .... The very streets of the cities became filled
> with visions of elegance. ladies 'like butterflies ... with their brilliant
> and vari-colored dresses, their glittering jewels, their air of sprightly
> and reckless extravagance.' Beside them stalked imposingly solid
> men: 'glossy-headed old nabobs with rubicund noses, bald foreheads,
> heavy side whiskers, portly bodies and great watch seals, types of
> prosperous sons of commerce; tnere are dapper little dandies and
> ponderous big dandies.'"

And elsewhere in the same work (2, page 651):

> "A gilded youth, James Hazen Hyde, eight years after the Bradley-
> Martin Ball at the Waldorf, gave one more brilliant at Sherry's which
> was transformed into the Palace of Versailles. But where Mrs. Mar-
> tin had chosen the period of Louis XIV, Hyde selected one with yet
> more unfortunate associations, and greeted his guests in the robes of
> the ill-fated Louis XVI. Promptly the people intimated that they knew
> history too, and were prepared to play their part, if drama was de-
> sired. Amid the general roar of rage, a sweeping investigation was
> started which uncovered startling frauds in the Equitable Life Insur-
> ance Company, a concern bequeathed to Hyde by his father. Hastily
> abdicating, the 'last of the Capetians' fled to France. But the popular
> storm, gathering volume, swept on to the muckraking and trustbusting
> explosion that carried Theodore Roosevelt to triumph."

Mark Twain gave the late nineteenth century a name which fit it exactly
and which has stuck to it ever since. It was "the Gilded Age," the age of
English clothes and accents, Roman orgies, continental travel, title-
mongering, art-collecting, butlers and footmen, conspicuous consumption
of every sort--the age which invented those now much more Americanized
institutions, the debutante and the society page. Not until the American
plutocracy had returned to its old habits of thrift and earthiness, of being
plain Americans, was there a return to relative civil calm in the United
States.*

More examples of the instability that ensues from the estrangement of
an elite are furnished in profusion by the Westernized elites of many cur-
rently underdeveloped areas. The elites referred to in this case are not
those who learn Western skills but remain identified with their native con-
text. Rather it is the visitors to Geneva and the riders in Cadillacs who
try to lead a life totally different from that of their people but who do not
segregate themselves completely from them. For estranged elites, living
abroad may indeed be a course preferable to the imitation of alien ways at
home; at any rate, they are in that case rather less conspicuous to their
people.

It is worth noting that, for the postwar period at least, the countries
in which internal wars have been relatively rare can be readily categorized
into two kinds of societies. They seem to be either relatively modernized
countries or relatively underdeveloped countries whose elites have re-
mained tied closely to the traditional types and structures of life. The
available data are shown in the following Table. The data in the Table are
crude and, in any case, a generalization of this kind is becoming increas-
ingly harder to test, since the number of societies without a gulf between
highly modernized elites and much less modernized masses seems to be
rapidly shrinking. Nevertheless, the notion is given credibility by the fact
that, while transitional societies seem to suffer more from internal wars
than either traditional or modern societies (as one would expect upon many
hypotheses), a very few seem to have strikingly low rates of violence com-
pared to the rest. Egypt is one example, and Pakistan another: these

---

*For evidence of acute unrest in the United States in this period, see
De Grazia (9, passim).

Table

Countries, grouped by level of development, for which there are less than five reports of unequivocal internal war in the New York Times Index for the period 1946-1959. More complete data appear in the Appendix to this chapter, pages 141-147.

Australia	Angola
Austria	Bechuanaland
Denmark	British Guiana
Finland	British Honduras
Luxembourg	British North Borneo
Sweden	Cameroons
Switzerland	Chad
Uruguay	Dominican Republic
West Germany	Eritrea
	Ethiopia
	French West Africa
	Gabon
	Gambia
	Guinea
	Liberia
	Saudi Arabia
	Somalia
	Surinam

societies seem to differ from the rest in one main respect. They have had "secondary" revolutions, so to speak, in which men of rather humble origins and popular ways (colonels' regimes) have unseated previously victorious transitional elites.

All this is not meant to validate the idea that elite alienation is the main cause of internal war but only to show why it should be taken very seriously. The possible consequences of elite alienation are not, however, the only reason for emphasizing studies of the incumbents at least as much as studies of insurgents in the etiology of internal wars.

Another reason is the fact that internal wars are almost invariably preceded by important functional failures on the part of elites; such wars, do not seem to arise solely from the power and aspirations of non-elites. Above all is this true of difficulties in financial administration--perhaps because finance affects the ability of governments to perform all their functions.* And finally, insurgent groups seem rarely to come even to the point of fighting without some support from alienated members of incumbent elites. On this point, agreement in the literature on internal war is practically unanimous.

---

*One of the most common conditions found before large-scale political violence is the financial bankruptcy of government, due to profligacy, over-ambitious policies, or the failure of a traditional tax structure in an inflationary situation, followed by an attack upon the financial privileges of strata which were previously the main props of the regime. Merriman (22) points out that the seventeenth century revolutions in England, France, the Netherlands, Spain, Portugal, and Naples all had this point in common.

Structural and Behavioral Hypotheses

A second strategic choice one can make in analyzing the causes of internal war is between structural and behavioral hypotheses. A structural hypothesis singles out what one might call "objective" social conditions as crucial for the occurrence of internal war: aspects of "setting," such as economic conditions, social stratification and mobility, or geographic and demographic factors. A behavioral hypothesis, on the other hand, emphasizes attitudes and their formation--not "settings," but "orientations" such as degrees of tension and anomie in societies and the processes by which tension and aggression are generated. The great majority of available propositions regarding the causes of internal war are, on the basis of this definition, structural in character (see this chapter, pages 116-117). In concentrating upon structural explanations, have writers on internal war followed the most promising tack?

At first glance, there would seem to be little to choose between structural and behavioral approaches to the causation of internal wars. Since most human action is motivated, not reflexive, one always wants to know, if one can, about attitudes underlying men's actions. At the same time, there would seem to be little doubt that attitudes are always formed somehow in response to external conditions. The difference between structural and behavioral theories would therefore seem to be, at best, one of emphasis, or point of ·· v, of looking at two sides of the same coin. Yet matters of emphasis can make a great deal of difference. Certain particular research results do seem to be associated with one point of view or the other. Behavioral approaches seem to lead mainly to theories stressing "intellectual" and voluntaristic factors in the etiology of political violence, such as conspiracy theories or theories attributing internal war mainly to efficient revolutionary indoctrination or inadequate value-formation by the incumbents. Structural explanations generally lead to theories attributing internal war mainly to specific situational conditions, attitudes themselves being treated as more or less mechanical responses to such conditions.

Which approach is preferable if one has to choose? Despite the fact that there is a danger that the behavioral approach might lead to näive conspiracy theory (the belief that internal wars are always the results of insidious indoctrination by subversive elements and could therefore always occur, or always be avoided), the arguments against a primary emphasis on structural theories are very strong.

One such argument derives from the general experience of modern social science. Purely structural theories have generally been found difficult to sustain wherever they have been applied. One fundamental reason for this is that patterns of attitudes, while responsive to the settings in which men are placed, seem also to be, to a great extent, autonomous of objective conditions, able to survive changes in these conditions or to change without clearly corresponding objective changes. This is one of the basic insights underlying the Theory of Action, which, to be sure, attributes an important role to the situations in which human action occurs but treats "culture" largely as a separate variable and assigns great significance to agencies of socialization and acculturation.

No doubt this point should be much elaborated. But one can make a cogent case for stressing behavioral theories of the causes of internal wars without going lengthily into the general nature and past experiences of social science.

The most obvious case for behavioral theories of internal war derives from the very fact that so many objective social conditions seem to be associated with it. We may have available so m. ny interpretive accounts of internal wars simply because an enormous variety of objective conditions can create internal war potential. Certain internal wars do seem to have followed economic immiseration; others, economic improvement. However, many more have followed some combination of the two. Some internal wars have in fact been preceded by great, others by little, social mobility. Some regimes have been more oppressive and others more liberal in the immediate prerevolutionary period; some both, and some neither. It is not reasonable to conclude that one should not seek explanations of the occurrence of internal wars in particular social conditions but seek them rather in the ways in which various social conditions may be perceived? Instead of looking for direct connections between social conditions and internal war, should one not look rather for the ways in which an existing cognitive and value system may change, so that conditions perceived as tolerable at one point are perceived as intolerable at another? Or, concomitantly, should one not look at the ways in which old systems of orientation are in some cases maintained rather than adapted in the face of social change, so that changes which one society absorbs without trouble create profound difficulties in another?

Perhaps some day research will uncover an objective condition which leads to internal war with very high probability and the opposite of which produces internal peace with equally high probability. But in light of what has been done so far, that would be surprising. Research so far suggests that almost any social condition can lead to internal war, and that in turn suggests that the crucial thing to know is not the conditions found in prerevolutionary societies but the orientations in terms of which such conditions are perceived and evaluated, or, more precisely, the interaction of conditions and orientations, with as much emphasis on the orientations as on the conditions.

The sad history of Marxist theory as a predictive model for internal war provides an object lesson in support of this view. Marxism singles out certain objective social conditions as underlying internal wars. It also singles out certain social groups as indispensable to the making of internal war. But Marxist revolutions themselves have been made neither under the social conditions nor by the groups emphasized in the theory. What is more, these revolutions have been made in a large variety of conditions, with a large variety of means, and by organizations constituted in a large variety of ways. This is true even if one can show that the appeal of Marxism is greatest in transitional societies, for the term transition, in its very nature. denotes not a particular social state but a great many different points on the continuum from tradition to modernity.

Particular Conditions and General Processes

This argument has a bearing also upon a third strategic choice which must be made in analyzing the causes of internal war. Even if one

emphasizes behavioral characteristics in theories of internal war, one
must of course always relate these characteristics to the social setting.
The question is whether, in dealing with the setting, one should develop
propositions emphasizing particular social conditions or propositions
about general characteristics of social processes. For example, the fol-
lowing general propositions might be cited: (a) those which relate internal
war to characteristics of the general phenomenon of social change itself,
to rapid change or erratic change, in whatever sectors such change occurs
(not those which relate internal war to any substantive social change) or
(b) those which relate internal war to the existence of significant imbal-
ances between sectors, such as imbalances between different elite groups
(e.g., between elites of wealth and elites of status) or imbalances between
the authority patterns of a society (not those which relate internal war to
any particular sector of society).

This choice can be dealt with more briefly than the others, for the
answer to it by now seems clear. The fact that so many particular social
conditions may be connected with internal wars—perhaps also the fact that
internal war is such a very common human experience—suggest strongly
that broad formulations about social processes and balances, which can
comprehend a large variety of particular conditions, should be stressed.
Undoubtedly there is a danger that such broad formulations will turn into
empty and untestable generalizations, trivialities like the much-repeated
proposition that political violence tends to accompany social change. But
this danger is avoidable. One can, after all, be specific about processes
and balances without specifying their substantive content.

OBSTACLES TO INTERNAL WAR

So far I have tried to make two related points. The first is that one is
most likely to gain an understanding of the forces impelling societies to-
ward internal war if one avoids one kind of analysis and emphasizes three
others. One should avoid preoccupation with the more visible precipitants
of internal wars, including conspiracies, and direct one's efforts to the
analysis of three aspects of their much less manifest preconditions: the
nature of incumbent elites, "behavioral" characteristics of society, and
the analysis of general social processes. The second point is, of course,
the converse of these positions: that our understanding of the etiology of
internal wars is dangerously inadequate precisely because studies have so
far concentrated on precipitants rather than preconditions, insurgents
rather than incumbents, and objective social conditions rather than social
orientations and social processes.

An important point must now be added. Even if we had better knowl-
edge of the forces which push societies toward political violence, a crucial
problem relating to the anticipation of internal wars would remain. This
problem concerns forces which might countervail those previously dis-
cussed: "obstacles" to internal war as against forces which propel soci-
eties toward violence.

In the real world of phenomena, events occur not only because forces
leading toward them are strong but also because forces tending to inhibit,
or obstruct, them are weak or absent. An automobile may generate a great
deal of force but if driven up a steep incline is unlikely to go very fast. A

government may have the desire and technical capacity for rapid indus-
trialization but if faced by the rapid growth of an already too great popula-
tion may simply find it impossible to channel sufficient resources into
capital goods to achieve a certain rate of development. So also internal
wars may fail to occur solely or mainly because of certain hindrances to
their occurrences.

Some of these hindrances may be absolute in character in that wher-
ever they exist internal war fails to materialize; hence their obverse may
be considered "requisites" of internal war (necessary, but not sufficient,
conditions). In the main, however, obstacles to internal war, like forces
making for internal war, are better conceived as factors making such
wars more or less likely, rather than either inevitable or impossible—the
significance depending, at least in part, on the strength of forces pulling in
a contrary direction. It certainly seems unlikely that we will ever find a
condition that makes internal war quite inevitable under any circumstances
and equally unlikely that we could discover conditions that always rule it
out (except perhaps purely definitional ones: e.g., the absence of any per-
ceived frustrations). In real life, internal war, like other concrete events,
results from the interplay of forces and counterforces, from a balance of
probabilities pulling toward internal war and internal peace.

The study of internal peace, hence of forces that inhibit political vio-
lence, should therefore be part and parcel of the study of internal war—not
only logically, but because the study of obstacles to internal war involves,
as we shall see, some extremely important policy problems.

Repression

The most obvious obstacle to internal war is, of course, the incumbent
regime. It goes almost without saying that by using repression the estab-
lished authorities can lessen the chances of violent attack upon themselves,
or even reduce them to nil. Internal wars, after all, are not made by im-
personal forces working in impersonal ways but are made by men acting
under the stress of external forces. This much at least there is in the
conspiracy theory of revolution: wholly spontaneous riots by wholly un-
structured and undirected mobs may occur, but hardly very frequently or
with much effect. Actual cases of internal war generally have some ele-
ment of conspiracy in them, some structure for forming political will and
acting upon decisions, however primitive and changeable. On this point, if
no other, the great enemies of revolution (Burke, Chateaubriand, Taine)
are at one with the great revolutionaries (Lenin, Trotsky); it is also this
point, rather than some more subtle idea, which underlies Pareto's (25)
and Brinton's (6) argument that revolutions are due to elites as much as
non-elites. And anything with a structure can of course be detected and
repressed, though not always very easily.

The matter, however, is not quite so simple. Repression can be a
two-edged sword. Unless it is based upon extremely good intelligence and
unless its application is sensible, ruthless, and continuous, its effects may
be quite opposite to those intended. Incompetent repression leads to a
combination of disaffection and contempt for the elite. Also, repression
may only make the enemies of a regime more competent in the arts of
conspiracy; certainly it tends to make them more experienced in the skills
of clandestine organization and sub rosa communication. No wonder that

botched and bungled repression is often a characteristic of prerevolutionary societies. The French ancien régime, for example, had a political censorship, but it only managed to make French writers into masters of the hidden meaning and to whet the appetite of the public for their subversive books. "In our country," a French aristocrat said, "authors compete with one another for honors of the bonfire"; even the queen seems to have spent many deliciously illicit evenings reading the Encyclopedia with her ladies.[*] Russia, under the later czars, was practically a model of repressive bumbledom; her exile policies, for example, created close-knit communities of revolutionaries more than destroying their cohesion.

The worst situation of all seems to arise when a regime, having driven its opponents underground, inflamed their enmity, heightened their contempt, and cemented their organization, suddenly relaxes its repression and attempts a liberal policy. The relaxation of authority is a part of the prerevolutionary syndrome no less than other forms of social amelioration; in that sense, repression in societies with high internal war potential is little more than a narcotic, intensifying the conditions it seeks to check and requiring ever larger doses to keep affairs in balance--if other things are equal. We can see this dynamic at work in the development of totalitarian rule, particularly if we remember that blood-letting, while certainly the ultimate in repression, is only one form that coercion can take.

From this standpoint, repression may be both an obstacle to and a precipitant of internal war. Repression is, of course, least likely to prevent internal war in societies which, unlike totalitarian regimes, have a low capacity for coercion. In such societies, adjustive and diversionary mechanisms seem to check revolutionary potential far better.

Diversions and Concessions

Diversionary mechanisms are all those social patterns and practices which channel psychic energies away from revolutionary objectives--patterns and practices which provide other outlets for aggressions or otherwise absorb emotional tensions. If Elie Halévy's (12) theory is correct, then English nonconformist evangelicalism, especially the Methodist movement, furnishes an excellent case in point. Halévy, being French, was deeply puzzled by the fact that England did not have any serious revolution in the early nineteenth century despite conditions which, on their face, seem to have contained very great revolutionary potential--conditions resulting from the industrial revolution and from the fact of endemic revolution throughout the Western world. His solution was that English evangelicalism, more than anything else, performed a series of functions which greatly lowered the revolutionary level of British politics. Among these functions were (a) the provision of outlets of emotional expression and (b) the inculcation of a philosophy which reconciled the lower classes to their condition, making that condition seem inevitable and making patient submission to it a sacred obligation. In England, at least at the time in question, religion seems indeed to have been the opiate of the people, as Marx and Engels, no less than later and different-minded historians, seem to have realized.

---

[*]For much information relevant to this point, see Taine (33).

England may have been spared major political violence (not political violence altogether, as we have seen) since the seventeenth century for other reasons too. She may have been spared, for example, because external war luckily occurred at least twice in English history, unifying the country, as external wars will, just when she seemed to be on the very brink of civil war. first, at the time of the Napoleonic wars and second, in 1914, after the mutiny in the Curragh threatened to develop into something much more serious. Indeed, diverting popular attention from domestic troubles by starting foreign wars is one of the most venerable dodges of statecraft. This too, however, is a weapon that can cut two ways. Military adventures are excellent diversions, and military successes can marvelously cement disjoined societies; but military failure, on the evidence, can hardly fail to hasten revolution in such cases. Russia may well have entered World War I to distract domestic unrest, but, if that were the purpose, the outcome was revolution rather than the contrary.

Orgiastic excitements—like festivals and dances, parades and circuses, Reichsparteitage and mass gymnastics—also provide diversionary outlets for popular discontent. "If the late czardom," says Edwards (10, page 49), "instead of abolishing vodka, had made it more plentiful and very cheap—if, in addition, they had stimulated to the utmost those forms of religious frenzy and excitement to which the Russian populace appear to be so susceptible—then it is at least possible that the people would have been so exhausted mentally, emotionally, and financially by their alcoholic and religious orgies that they would not have had sufficient energy left to carry out a successful revolution."

Totalitarian regimes seem to be shrewder about such matters, as well as more coercive. The massive sports programs which are a feature of every totalitarian regime (German, Russian, or Chinese) may have a variety of purposes—for example, physical fitness as preparation for war or the inculcation of discipline—but one of them assuredly is to absorb the energies of the young and the interest of the not-so-young. No less than eschatological ideology, sport is the opiate of the masses in totalitarian countries, and not in these alone.

Adjustive mechanisms reduce, or manage, tensions, rather than providing for them surrogate outlets. Concessions are perhaps the most obvious of such mechanisms. It is banal, but probably true, to say that timely concessions have been the most effective weapons in the arsenal of the British ruling class. One of Halévy's (12) more cogent points about the pacific effects of evangelicalism on nineteenth-century England is that it made the elite extraordinarily willing to ameliorate the lot of the masses. It enjoined upon them philanthropy as a sacred duty and educated them in the trusteeship theory of wealth (remember Wesley's counsel, "Gain all you can, save all you can, give all you can") at the same time that it made the masses extraordinarily willing to suffer their burdens in peace. (For this reason, we can of course regard all functioning institutions for adjusting conflicts as barriers to internal war.) But concessions may work in two directions, no less than repression and certain diversionary tactics. They may only lead to further and greater demands, further and greater expectations of success, and must therefore, quite like repression, be continuous, and continuously greater, to succeed. "There is no better way (than a concilitory policy)," according to Clemenceau (quoted in Sorel, 30, page 71), "of making the opposite party ask for more and more.

Every man or every power whose action consists solely in surrender can
only finish by self-annihilation. Everything that lives resists ...." A
search for the conditions and attitudes which favor adjustment on the one
hand or appeasement on the other as products of concessions may prove
rewarding.

The Capacity for Violence

In the final analysis, the surest obstacles to internal war, apart from
orgiastic diversions, are a series of conditions not yet discussed: those
which affect the capacities of alienated groups to use violence at all, or,
more often in real life, to use it with fair prospects of success. These
conditions do not always prevent violence. But they can prevent its suc-
cess. For this very reason, they help determine the likelihood of deci-
sions to use violence. What are some of these conditions?

Perhaps the first to come to mind is terrain. While practically all
kinds of terrain can be used, in different ways, for purposes of rebellion,
not all can .ie used to equal advantage. The ideal, from the viewpoint of
the insurgents, seems to be an area which is relatively isolated, moun-
tainous, overgrown, criss-crossed by natural obstacles (hedges, ditches,
etc.), and near the sea or other sources of external supply. Such terrain
affords secure bases to the insurgents in their own territory, gives them
the advantage of familiarity with local conditions, and allows ready access
to them of external supporters.*

The communications facilities of a society are another relevant con-
dition. Marx, among many others, seems to have realized this when he
argued that urbanization increases the likelihood of revolution, if only in
that it makes men accessible to one another and thus makes revolutionary
organization easier to achieve. "Since the collective revolutionary men-
tality is formed by conversation and propaganda," writes the French his-
torian Lefebvre (19, page 23) "all means that bring men together favor it."
In this one case, a condition which may heighten the chances of successful
internal war (bad communications) may also discourage its outbreak.
There may be nothing more mysterious to the celebrated peaceability cf
peasants, as compared to city dwellers, than the physical difficulty in
rural life, especially if fairly primitive, to form a "collective revolution-
ary mentality."

Terrain and communications are physical obstacles to (or facilities
for) internal war. There are human obstacles as well. For example, in-
ternal wars seem rarely to occur, even if other conditions favor them, if
a regime's instruments of violence remain loyal. This applies above all
to the armed forces. Trotsky (35, page 116) for one, and Lenin for an-
other, considered the attitude of the army absolutely decisive for any rev-
clution; so also did Le Bon (18, page 29). Pettee (27, page 105), on the
other hand, dissents, but for a rather subtle reason: not because he con-
siders the attitude of the armed forces insignificant, but because he feels
that armies never fail to join revolutions when all other causes of revolu-
tion are present and that they never fail to oppose them when this is not

---

*For examples of how such terrain benefits insurgents, see among
many others, Paret (24), Allen (1, page 19), Johnson (14), and Guevara (11).

the case. We could enlarge this point to read that internal wars are un-
likely wherever the cohesion of an elite is intact for the simple reason that
insurgent formations require leadership and other skills and are unlikely
to obtain them on a large scale without some significant break in the ranks
of an elite. Even if elites do not always "cause" their own downfall by be-
coming rigid or foreign to their people, they can certainly hasten their own
demise by being internally at odds. From this standpoint, if not from that
of Mosca's (23) theory, elite cohesion is a factor which should be classi-
fied among the obstacles to internal war, rather than among their causes.

A final human obstacle to internal war, perhaps the greatest of all, is
lack of wide popular support for rebellion. It seems generally accepted
among modern writers on internal war--indeed it is the chief dogma of
modern revolutionaries--that without great popular support the insurgents
in an internal war can hardly hope to win--unless by means of a coup--and
that with such support they are hardly likely to lose. So vital is this factor
that some writers think that the distinctive characteristic of internal war
is the combination of violent techniques with psychological warfare, the
latter designed, of course, to win the active support of the noncombatants.
This is asserted in the much repeated pseudo-formula of the French theo-
rists of guerre révolutionnaire: revolutionary warfare = partisan war +
psychological warfare.* To be sure, psychological warfare occurs now-
adays also in international wars. Its role in these, however, is not nearly
so crucial as in internal war; it is incidental in international war but, with
the exception of the coup, seems to be decisive in internal war.

One reason for this is that in internal wars, unlike international wars,
there is generally a great disparity in capacity for military effort between
incumbents and insurgents. The former tend to be in a much stronger po-
sition. This is not always true, of course, for this is where the loyalties
of the established instrumentalities of violence enter the picture. The in-
surgents are therefore forced, in the normal case, to supplement their
capabilities by taking what advantage they can of terrain and of the coop-
eration of the noncombatant population.

Like terrain itself, a well-disposed population affords a secure base
of operations to rebels and provides them with indispensable logistical
support. Rebels who can count on popular support can lose themselves in
the population,** rely on the population for secrecy (in wars in which in-
telligence, as I have pointed out, is practically the whole art of defense),
and reconstitute their forces by easy recruitment. If the rebels can do all
of these things, they can be practically certain of victory, short of a resort
to genocide by the incumbents.

Great popular support is necessary also in internal wars, because the
common disparity of forces rules out quick victory by the insurgents (ex-
cept by coup). They are often either prolonged wars of attrition or mat-
ters of only a few hours. In prolonged wars, when victory always seems

---

*See Bonnet (5, page 60). The point that in guerrilla warfare almost
everything turns on popular support is argued in many sources, most
strongly perhaps by Johnson (14).
　**According to Mao, "the populace is for revolutionaries what water
is for fish.'

remote, when, at times, impasse is the best that can be hoped for, when
the disruption of normal life is greater even than in external war, the mo-
rale of the revolutionaries, their ultimate trump card against their oppo-
nents, can hardly be sustained if they feel themselves isolated from their
own people.

For all of these reasons, calculations about pop·lar loyalties nor-
mally play a role in the decision to resort to political violence. The cal-
culations may be mistaken, but they are almost always made; sometimes,
as in the case of the Algerian nationalist struggle, they are made in ways
approaching the survey research of social science.*

A PARADIGM FOR ESTIMATING INTERNAL WAR POTENTIAL

We can summarize these remarks in the form of a paradigm for esti-
mating internal war potential. A paradigm, it should be noted, is not a
theoretical model. Its terms do not have the precision and its structure
does not have the logic such a model should have; in a sense, it is an im-
precise first approximation to a model. At bottom, it merely states the
variables to be considered in the analysis of a problem and the more cru-
cial questions it raises--the approximate form which analysis of the prob-
lem should take.

Nevertheless, at the risk of being accused (correctly) of pseudo-
mathematics, I shall state the paradigm here in the form of what looks
like an equation, but only for two reasons: (a) for the sake of vivid repre-
sentation and (b) because the discussion has taken an obviously mathemat-
ical turn, some elements in the discussion being considered positive in
relation to the occurrence of internal war and others, negative. This
makes it possible to think of the forces involved in generating internal war
as follows:

$$W = f \frac{(Cer. I, Fi)}{(Adi, R, E, Fr)}$$

In this formulation, W stands for internal war potential--since this is
all we can assess if internal wars never or rarely occur unless precipi-
tated by events beyond the scope of systematic analysis. The terms above
the line stand for forces making for internal war; those below, for coun-
terforces. (C) denotes social changes found relevant to the occurrence of
internal wars, or any social change at all, if it turns out that any change in
the structure of society is a condition favoring the occurrence of internal
wars--as seems probable at this stage of inquiry. (Ce denotes the exten-
siveness of relevant changes and (Cr), the rate of change, since, presum-
ably, rapid changes will have effects more unsettling and volatile than slow
ones. (I) denotes the existence of imbalancing mechanisms in a society,
mechanisms which can intensify the strains arising from social change.
Conspiratorial organizations come under this category, as do any other
institutions which tend to make men more aware of strains or more re-
sentful of them. One might also include under this category particular

---

*Interview with M. Chanderli, F. L. N. (Observer at the United Na-
tions). December, 1961.

governmental acticns which tend to intensify alienation, su᠆h as ccaces-
sions granted and withdrawn, or repressive measures, sufficient to create
conspiratorial organization but insufficient to countervail them, applied
and sucdenly relaxed while the revolutionary organizations are still func-
tioning. (Fi) denotes the facilities available to the insurgents for fighting
internal wars such as terrain, skilled leadership, and popular support
which decide their capacity for warfare; (Fr) denotes the counter-facilities
available to the incumbent regime. (E) denotes the degree of elite cohesion
in a society; (R), the extent of effective repression exercised by the in-
cumbents, and (a), the various adjustive and diversionary mechanisms
available in the society--(Ad) denoting those deliberately adopted by the·
regime, such as concessions, and (Ai) denoting those already given in the
institutional structure of society, Jike organizational means for resolving
conflicts, social mobility patterns, or any other tension-managing mech-
anisms.

One could, of course, state the same things in ordinary prose--indeed,
one has to. But the form of the statement hardly matters. The essential
thing is to state the relevant variables, both those that act as inpelling
forces and those that act as obs᠆acles, however complicated a statement
that might require. Great analytical simplifications, whatever their utility
in abstract inquiry, are hardly ever adequate for the anticipation of con-
crete events or adequate policy postures in regard to thᴇm.

Approaching the problem of the generation of internal wars in such a
complicated fashion has an additional advantage. It makes it possible to
cope with a serious problem raised at the outset of the discussion but
avoided thereafter· how to deal in disparate, yet not totally unconnected,
ways with the very different phenomena described by the term internal
war--riots, coups, political revolutions, social revolutions, and wars of
independence, to follow only one possible classificatory scheme (see this
chapter, pages 103ff).

From the fact that internal wars take so many different forms, some
writers infer that each type of internal war requires a separate theory: a
separate set of causes and obstacles, a particular theory of development,
and special theories regarding outcome and effects. One important theme
in this discussion is that the social conditions which can be related to in-
ternal war, either as causes or as obstacles, do vary widely. Might it not
be that this is due to the fact that the kinds of internal war also vary
widely? In addition, it has probably been apparent throughout this discus-
sion that some arguments stated in general terms do not apply with equal
force to every particular internal war. Take, as an obvious example, the
argument against the conspiracy theory of internal war. This certainly
does not apply in the case of coups d'état even if it applies in other cases
of internal war. Can there in fact not be a kind of internal war in which
conspiracᴊ is all-important, while in others all elements of preparation,
or organization, planning, and direction, are lacking?

The paradigm sketched above provides a means for skirting these dif-
ficulties in that it makes it possible to deal with the variety of events which
constitute internal war within a single framework. By weighting the bal-
ance of forces above and below the line, one can arrive at some judgment
about the overall degree of internal war potential in a society. By looking
at the particular forces which are strong or weak, one can arrive at some

idea of what kind of internal war might occur. For example, where (Cer) is great but (I) and (Fi) small (disregarding conditions below the line), one might expect internal war to take the form of riots. Again, where (Cer) and (E) are small but (I) is great, the most probable result might be a coup d'état. If (Cer) and (I) are great, (E) small, and the ratio (Fi:Fr) nearly equal, one might expect full-scale civil war. One could, in fact, construct a special typology of internal wars in terms of the various possible balances of forces included in the paradigm; and one could equally well take any typology otherwise worked out and produce for it, potentially at least, counterparts stated in terms of the model. The main advantage this procedure has is that it can prevent the ad hoc piling up of totally unrelated theories, while not overlooking the role of particular forces in particular cases.

It is also easy to introduce international forces into the paradigm, even though the factors discussed here have all been stated as "domestic" forces. All that is required is that exogenous forces should be treated in terms of their endogenous effects: as special kinds of social changes which involve internal war potential (e.g., changes in the international position of a society); as forces that may "imbalance" societies, by example or by direct contributions to conspiratorial organizations; as facilities for either side, through the provision of technical means, leadership, secure bases, or facilities for regroupment; or as adjustive mechanisms, through the provision of diversions from domestic problems.

RESEARCH TOPICS: SOME ILLUSTRATIONS

There is practically no limit to the research that can be, and ought to be, undertaken on the subject of internal war. In a sense, the study of internal war is commensurate with the whole study of society, even peaceable society, for anything that increases our knowledge of social order can potentially increase our understanding of civil disorder. For this very reason, however, the problem of determining research priorities looms larger in this area of inquiry than in most others.

These priorities must vary with the purposes of the inquirer and the means available to him. They also depend upon the state of internal war studies at a given point in time. Research projects which are ideally desirable may not be very rewarding until less desirable research has been carried out. The suggestions below, consequently, are very far from exhaustive and are made with an audience in mind which is only partially academic. They are a small, merely illustrative, sample of two kinds of projects: (a) projects which can provide knowledge urgently needed for policy purposes, even if this knowledge must remain very insufficient in the foreseeable future and (b) projects which it would be particularly sensible to pursue in the present stage of inquiry and which can be carried out well only by means not normally available to social scientists working in their usual ways, either because of lack of money, or personnel, or apparatus, or access to information which government departments (presumably) possess. The hope is that government, with its large and unusual resources, will see fit to supplement normal social science research with support for just such projects.

Policy-Oriented Research

The following research topics illustrate the kinds of projects needed for purposes of intelligent and effective policy vis-a-vis internal war situations:

(a) <u>Analysis of the uses of internal war situations</u>. Since internal war situations are often largely inchoate in their initial stages; since they are very difficult to anticipate correctly under the best of circumstances, owing to the role of ephemeral precipitants in bringing them about and the complexity of their preconditions; and since they are particularly difficult to anticipate correctly with the knowledge presently available; nothing would seem to be more urgently necessary than knowing how to use them for one's own policy purposes once they have occurred. We live in a revolutionary world in which internal war is a basic fact of life. In such a world even conservative powers need conspiratorial theories; they can hardly hope to contain the tide of revolution everywhere, especially while being in the dark about the forces causing it. Studies of the techniques by which internal wars can be molded and channeled are therefore of the utmost importance. The Machiavellian overtones, the apparent cynicism, may make such studies repellent, but that cannot be helped.

(b) <u>Studies of communist theories of internal war</u>. No doubt we can learn a good deal about using internal wars from the communists, who are masters of that unattractive art. But this is only one of many reasons for studying communist ideas about internal war. Without subscribing to the theory that communists can make internal wars under any and all conditions, or the view that modern internal wars are all communist-inspired, one can nevertheless argue that knowledge of communist ideas about the preconditions of internal war is indispensable at present for anticipating particularly crucial internal wars--those in which communists are in fact involved. The communists undoubtedly have a theoretical model of internal war (or several, depending on whether we are concerned with Russian, Chinese, or various satellite versions of the creed) based on their fundamental philosophical principles as amended by reflection upon much successful and unsuccessful experience with political violence. To anticipate their activities (and also to learn from them), we ought to know their relevant ideas--about the preconditions of internal war, about the ways in which they can be precipitated, and about the strategies and tactics employed in fighting them. No doubt officers of government are constantly engaged in such appraisals; it is inconceivable that they might not be. But the very nature and urgency of the subject makes it particularly desirable that the inevitably harrassed, hurried, anxious and case-bound appraisals of government officials should be supplemented by the more dispassionate, more systematic, larger appraisals of social scientists. Precisely because they are not so close to urgent policy problems their interpretations might prove to be the more apposite.

(c) <u>Studies of the efficiency of certain policy responses to internal war potential</u>. Here the most important subjects are to determine the optimum uses of repression, diversion, and concession, or combinations of the three. Each of these responses to internal war potential is potentially useful and potentially dangerous. Each has worked and failed in certain situations. Under what conditions, structural and behavioral, are they likely either to work or to fail? How far should they be carried? What

kinds of repression, aiversion, or concession work best in what sort of situations? How can one policy response best be combined with another -- repression. for example, with concessions or diversions?

(d) Studies of the internal war potential of critically sensitive areas. In general, the systematic study of internal war potential must be a long drawn-out process; there are no short cuts to knowledge as inherently complicated and difficult to acquire as knowledge of this subject. In certain countries, however, we can hardly await fully adequate theoretical knowledge to carry out concrete appraisals. Such countries are those in which the security of important military bases might be imperiled by internal war or countries in which internal war might imperil our fundamental international designs. For example, internal war in a Western European country might seriously endanger our alliance system or our promotion of Western economic and political unity. However inadequate our present means, we do have some available for carrying out relatively close analyses of the situations in such countries. Indeed, such close analyses of particular situations should be a continuous process, in which all analytical means at hand are constantly used to make and revise appraisals, while the appraisals are used to improve the analytical means.

No doubt close analyses of such areas are in fact constantly made in the government, but the impressions and intuitions of those who make them should be supplemented by, and tested against, the more detached appraisals of social scientists. For the latter, the pay-off of such exercises would be the constant testing of their appraisals against empirical experiences--predictive exercises which might confirm or invalidate their theoretical approaches.

The suggestions above illustrate policy-oriented research, much of which would profit greatly from the availability of data not ordinarily available to independent social scientists. One could think of many more examples, of course, particularly in the area of operational theory. I have not spelled out any of these because I strongly believe in the priority of the problem of anticipation and because others are more expert on operational questions. As for other projects relating to the problem of anticipating internal war, these can easily be der'ved from the text of the study.

The Study of Symptoms of Internal War Potential

However, I should like to outline one other topic for research, to illustrate the kind of general-purpose inquiry requiring large resources which might be particularly useful at this stage of inquiry. This topic involves inquiring very broadly into what might be called "symptoms" (or "indicators") of internal war potential.

A symptom of internal war is a condition which indicates, with a high degree of accuracy, that internal war will occur in a society's future. Symptoms are not synonymous with causes. To be sure, in some cases we may call something a symptom of internal war simply because we do not yet understand (or are not concerned with) its causal relation to the occurrence of such wars. Normally, however, the term refers to conditions which merely indicate, or are presumed to indicate, the existence of the causes themselves.

This raises the question why one should bother about symptoms when one could proceed directly to the analysis of the causes which they indicate. There are at least three good reasons for doing so. (a) Etiology is notoriously difficult in the social sciences, if not in all fields of inquiry; a decent "symptomatology" of internal wars may permit one to anticipate with reasonable accuracy when and where such wars are about to occur, even before one knows much about their causes. (b) Knowledge of conditions which appear frequently to presage internal wars may help one to look for their causes in the right places instead of going off on pointless forays among irrelevant conditions. The search for symptoms of internal wars is thus, in a sense, logically prior to the search for their causes, and particularly useful in the early stages of analysis. (c) If one confines discussion at an early stage of inquiry to questions of symptomatology, one can avoid complicated questions of human motivation (what alienates people from their societies, what drives them to seek aggressive release from their frustrations) and concentrate on problems requiring more modest theoretical equipment and techniques. One can, in fact, confine oneself to the simplest application of comparative method, the examination of numerous internal wars in order to discover whether any condition or set of conditions precedes many or most of them.*

Although the literature on indicators of internal war potential is very small, it is possible to illustrate the sort of studies which might be done, and the sort of knowledge which might be turned up, from existing outdated (but still fascinating) studies produced by the positivist school of sociology. The outstanding example, by far, is L. P. Edwards' The Natural History of Revolution (10).**

Edwards was interested in symptoms of internal war not merely because he was something of an old-fashioned positivist, a man who preferred the relative incontrovertibility of "natural history," the recording of data and coincidences among data, to the more debatable questions of causality or functionalism. He was interested in discovering early symptoms of internal war also because he believed that revolutions in fact never happen very suddenly and are never themselves the principal agents of social change. Revolution, he argued, is always the result of a long evolution, a product, and perhaps final ratification, of important social changes which precede it and of the failure of existing elites to recognize the implications of such changes. Of all social movements, in his view, revolutions are the slowest in the forming, none having occurred with less than three generations of preparation (except only for countries in which civil strife is chronic). Such a view is necessary if one is to believe in the feasibility of detecting internal war potential long before the actuality; and if the view is justified, it provides still an additional reason for attempting to construct a symptomatology of internal wars, even by the examination of social conditions long in advance of civil violence.

---

*Cf. Knorr's discussion of the usefulness of indicators (symptoms), pages 84 - 88, above.

**Other works which include useful comments on the symptoms of internal war are by Brinton (6), Sorokin (31), and Pettee (27).

What then are some of the social conditions which have been identified by one writer or another, as symptoms of internal war? Edwards (10)* believed that the very earliest symptom of internal war is widespread restlessness in a society, and this in the most literal sense. Rates of travel markedly increase, so does the number of tramps and hobos, and so does the rate of emigration (though emigration, above a certain level, may help to prevent the actual occurrence of internal war by siphoning off alienated people). Such physical mobility may have a causal feedback on the stability of society by acquainting men with shortcomings in their domestic condition and with possible alternatives, but initially it is merely a sign of unsettled disposition and quite haphazard in character. Physical restlessness of this sort may no longer be a very useful indicator of internal war potential (if indeed it ever was), since it may now be little more than a reflection of modern life, of a heightened capacity for mobility provided by modern transportation facilities and the loosening of particularistic ties. Nevertheless in primitive or relatively underdeveloped societies it may reflect even today what Edwards thought it reflected in the past: social dissatisfactions as yet unrealized and unfocused, leading to behavior which can produce a clearer understanding of the dissatisfactions and of possible remedies for them. Even in the more developed societies, however, certain kinds of "restlessness" may indicate internal war potential. Edwards points out, for example, that internal wars seem to be preceded by a considerable increase in dissolute behavior, by a widespread foraging for new mores, by attempts to escape from staleness and boredom in aimless sensation-seeking--in a word, by the compulsive pursuit of stimulation. And Sorokin (31, pages 41-42) points out that civil strife seems always to be preceded by another form of "restlessness" which might occur in any kind of society: a general "loosening of tongues," as reflected in greatly increased verbal and literary output of all sorts, particularly, of course, of political pamphlets, placards, manifestoes, and so on.

Other symptoms of internal war potential might be found in certain characteristics of social thought in prerevolutionary society. Most important perhaps--Edwards (10, page 38) calls it the "master symptom" of revolution--is a general "transfer of allegiance" on the part of the society's

---

*Edwards (10, pages 25-26) writes:

"The wandering university students of the thirteenth and fourteenth centuries broke the road for the Renaissance, but they never knew they were doing it. Of the countless thousands of knights and barons who went on the Crusades few or none perceived how those military wanderings were to aid in overthrowing the feudal system. The inveterate peripatetic habits of Greek and Roman philosophers, more especially in the two or three generations preceding the overthrow of the Roman Republic, are well known . . . .

"The French philosophers of the first three quarters of the eighteenth century held a record for peregrination. Voltaire's travels had a marked influence on the development of French political thinking in the period preceding the French Revolution. The other philosophers were not much behind Voltaire . . . .

'Russians were notable for their traveling propensities a full century before the revolution of 1917. The present (1927) government of Russia is composed of men who have traveled or wandered over nearly every country on earth . . . ."

intellectuals (its authors, editors, lecturers, artists, teachers, priests, and preachers, "all those whose function it is to form and guide public opinion"). This does not necessarily manifest itself in outright revolutionary activity on their part. It may also occur in the form of a withdrawal from politics; the indifference of the clercs to the established political order causes their failure to serve as "symbol specialists" and to perform for the political order certain obviously necessary "socialization" functions.

In the earlier stages of the development of revolutionary situations, one notices little outright intellectual assault on established institutions. One finds rather a greatly increased interest in the working of society, a growing sociological orientation on the part of intellectuals of all sorts--a compulsive concern with the nature of society rather than with outright alienation. This seems often to be accomplished by a considerable growth of scandal-mongering, of muckraking and literary exposure, at first directed mainly at individuals, then assuming increasingly the character of a debunking of institutions. Muckraking finally turns into something resembling oppression psychosis: there develops a literature of hatred against a real or imaginary enemy to whom a sort of evil omnipotence is attributed. Thought, in other words, increasingly becomes sociology, and sociology turns into a literature of scandal and exposure, hatred and paranoia.

One could go on in this vein to describe many other characteristics of social life which one writer or another has found typical of prerevolutionary situations. Before the occurrence of internal wars, societies seem frequently to be afflicted by administrative entanglements and confusion; by, as Brinton (6, pages 37-41. 52-67, 68) puts it, a "loss of self-confidence among many members of the ruling class"; by a conversion of many members of that class to the view of its critics; by important stoppages in professional and white-collar careers, and by a growing discrepancy between power (especially economic power) and status.

But there is no point here in dwelling on these "symptoms" at great length. The object is not to present an actual symptomatology of internal wars but to illustrate (a) what might conceivably be done at this level of inquiry, (b) how such inquiry might permit one to gauge internal war potential without close knowledge of the particular forces producing it, and (c) how it might also point the way toward the discovery of these forces.

The utility of an inquiry of this type will be small if it is not carried out on a very large scale (through the comparative examination of practically the whole of social life in prerevolutionary societies) and with great patience--in other words, with resources not normally available in the social sciences. Lacking such resources, one has recourse to studies like those of Edwards which brim over with suggestive possibilities but which are woefully inconclusive.

Rightly conducted, a general inquiry into prerevolutionary conditions could produce an urgently needed generalized resource for internal war studies: a large library of well-codified materials (perhaps on IBM cards) which could be used for comparative studies of every description relating to internal war. Theorizing--especially the adequate testing of theories-- is presently impeded by nothing so much as the chaotic nature of relevant

materials, uncollected or incoherently collected. In studying internal wars we are still on the threshold of what Northrop calls the "natural history stage of inquiry," the earliest stage of any systematic inquiry, in which one collects data in an orderly manner for theoretical processing. Because of the resources required, we may never get far even into that early stage of inquiry without an act of faith by someone with a very large stake in adequate internal war studies; and if we do not get far into it our theories about internal war will always have a regrettably and unnecessarily high degree of tentativity and intuition about them--as will our policies.

Given the urgency of present problems of internal war, there will be an understandable tendency to emphasize short-run concerns over projects which can yield results only in a rather exhaustive long run. But internal wars will always be with us and pose constantly shifting problems of ephemeral urgency. Indeed, if my analysis is correct, the incidence of internal wars and the urgency of the problems it poses will increase rather than diminish. In view of this, it might be worse than unfortunate if, searching for somewhat more adequate immediate policy postures, we prejudice the chances of much more intelligent responses to internal war situatio s in the future.

REFERENCES

1. Allen, W. E D. Guerrilla War in Abyssinia. New York: Penguin Books, 1943.

2. Beard, Miriam. A History of the Business Man. New York: Macmillan Co., 1938.

3. Benson, Lee, and Strout, Cushing. "Causation and the American Civil War. Two Appraisals," History and Theory: Studies in the Philosophy of History, 1, No. 2 (1961), 163-185.

4. Blanksten, George I. "Revolutions," in Government and Politics in Latin America, ed. Harold Eugene Davis. New York: Ronald Press Co., 1958.

5. Bonnet, Gabriel. Les guerres insurrectionelles et révolutionnaires. Paris: Payot, 1958.

6. Brinton, Crane. The Anatomy of Revolution. New York: W. W. Norton & Co. Inc., 1938.

7. _____. A Decade of Revolution, 1789-1799. New York: Harper and Bros., 1934.

8. Davis, James C. "Toward a Theory of Revolution," American Sociological Review, 27 (1962), 5-19.

9. De Grazia, Sebastian. The Political Community, A Study of Anomie. Chicago: University of Chicago Press, 1948.

REFERENCES (Continued

10. Edwards, L. P. The Natural History of Revolution. Chicago: University of Chicago Press, 1927.

11. Guevara, Ernesto. Che Guevara on Guerrilla War. New York: Frederick A. Praeger, 1961.

12. Halévy, Elie. A History of the English People. (Paperback edition, 6 vols.) London: T. F. Unwin, Ltd., 1960.

13. Huntington, Samuel P. (ed.). Changing Patterns of Military Politics. New York: Free Press of Glencoe, 1962.

14. Johnson, Chalmers. "Civilian Loyalties and Guerrilla Conflict," World Politics, 14 (1962) 646-661.

15. Kornhauser, William. The Politics of Mass Society. New York: Free Press of Glencoe, 1959.

16. Kropotkin, Pierre. Paroles d'un révolté, ed. Élisée Reclus. Paris: Marpon, 1885.

17. Lasswell, Harold D., and Kaplan, Abraham. Power and Society. New Haven, Conn.: Yale University Press, 1950.

18. Le Bon, Gustave. The Psychology of Revolution (tr. by Bernard Miall). London: T. F. Unwin, 1913.

19. Lefebvre, G. "Foules Révolutionnaires," Annales Historiques de la Révolution Francaise, II (Jan.-Feb. 1934), 1-26.

20. Malaparte, Curzio. Technique du Coup d'État. Paris: B. Grasset, 1931.

21. Merriam, Charles E. Political Power. New York: McGraw-Hill Book Co., Inc., 1934.

22. Merriman, Roger B. Six Contemporaneous Revolutions. Oxford, England: Clarendon Press, 1938.

23. Mosca, Gaetano. The Ruling Class (tr. by Hannah D. Kahn). New York: McGraw-Hill Book Co., Inc., 1939.

24. Paret, Peter. Internal War and Pacification: The Vendee, 1793-1796. Princeton, N.J.: Center of International Studies, 1961.

25. Pareto, Vilfredo. The Mind and Society. New York: Harcourt, Brace and Co., 1935.

26. Parsons, Talcott, et al. Theories of Society: Foundations of Modern Sociological Theory. New York: Free Press of Glencoe, 1961.

27. Pettee, George S. The Process of Revolution. New York: Harper and Bros., 1938.

REFERENCES (Continued)

28. Pressly, Thomas J. Americans Interpret Their Civil War. Princeton, N.J.: Princeton University Press, 1954.

29. Rudé, George F. E. The Crowd in the French Revolution. Oxford, England: Clarendon Press, 1959.

30. Sorel, Georges. Reflections on Violence. Glencoe, Ill.: The Free Press, 1950.

31. Sorokin, Pitirim. The Sociology of Revolutions. London: J. B. Lippincott Co., 1925.

32. Stokes, W. S. "Violence as a Power Factor in Latin American Politics," Western Political Quarterly, 5 (1952), 445-468.

33. Taine, Hippolyte. The Ancient Régime. New York: H. Holt and Co., 1896.

34. Tocqueville, Alexis de. De Tocqueville's L'ancien Régime. (tr. by M. W. Patterson). Oxford, England: B. Blackwell, 1933.

35. Trotsky, Leon. The History of the Russian Revolution. Ann Arbor: University of Michigan Press, 1957.

36. Webster, Nesta H. Secret Societies and Subversive Movements. London: Boswell Printing & Publishing Co., Ltd., 1924.

APPENDIX

INCIDENCE OF INTERNAL WARS, 1946-1959

The data in the table below came from the reports of internal wars in the New York Times for the period 1946-1959 as listed in the New York Times Index. A few of the more well-known internal wars of extended duration, such as the Indo-Chinese conflict, have been omitted, as have the Communist takeovers in the satellites following World War II.

Definitions of the column headings in the table are:

Equivocal-Unequivocal (abbr. E and UE respectively). The distinction is one between cases of clear-cut internal wars and cases which are ambiguous for these reasons: (1) effective countermeasures by the incumbent power holders minimized or precluded actual violence, e.g., abortive insurrections or coups; (2) cases for which the Times Index coverage was so inadequate that the presence of violence was difficult to determine; (3) cases which took the form of police or administrative actions which may or may not have involved outright violence, e.g., police round-ups of political opponents, or Soviet-type purges; (4) the violence was apparently not directed at the incumbents in the first instance, e.g., cases of inter-tribal disputes.

Warfare. This category includes both civil and guerrilla warfare, which have not been separately counted because of the frequent inadequacy of the source. Like external wars, both are characterized by a high degree of organization of the opponents, the continuity of fighting, the presence of operational planning, and the existence of territorial control, extended or discontinuous, by the insurgents.

Turmoil. Simultaneous, continuous rioting of considerable duration in two or more distinct geographic areas.

Rioting. Relatively unorganized and spontaneous short-term incidents, typically involving police contingents and an unintegrated mass whose objectives are somewhat modest. Frequently, however, the actual instigators are highly organized extremist groups.

Large-Scale Terrorism (abbr. L.S.) is the systematic use of intimidation and harassment by assassination and/or sabotage by relatively small but cohesive groups.

Small-Scale Terrorism (abbr. S.S.) is distinguished from the above by a more undisciplined and discontinuous use of terror, and by the occasional assassination or bomb plant.

Mutiny. Violence on the established order by groups which are part of its own instruments of force, such as the police, military, etc.

Coup. Violence or the threat of it by one or more parts of the elite against other parts, i.e., Lasswell and Kaplan's "palace revolution."

Equivocal Plots. These are equivocal either because they are exposed while in an early conspiratorial stage (and thus are not violent) or because the alleged plot may be only a pretext by which the government seeks to eliminate its political competitors.

Administrative Action. The removal of political opposition through the use of the formal administrative apparatus, as in communist-type purges, police round-ups or raids.

Quasi-Private. Cases which are equivocal internal wars because the violence was not initially directed at the government, or which appear not to be anti-government because of insufficient information. The Times Index gave a very high number of such cases for South Africa, but these so-called inter-tribal disputes are very often genuine internal wars.

The numerical footnote references have the following meaning:

[1] No attempt has been made here to produce a numerical index of internal violence since news coverage is so unreliable. Large-scale purges and anti-government violence have been included when the information seemed reliable.

[2] The numerical total here is considered a poor index of internal violence, since the element of duration of a prolonged civil war, for instance, is not reflected in these unweighted totals.

[3] This numerical index for internal violence in Cuba is distorted by the high incidence of small scale terrorism. Each incident was counted separately only because of the lack of information which would have enabled the compiler to aggregate them down in one or more large-scale terrorist campaigns.

[4] Almost all the violence reported here was perpetrated in Northern Ireland by Republican partisans.

INCIDENCE OF INTERNAL WARS, 1946-1959

	Totals		Unequivocal Cases (UE)							Equivocal Cases (E)		
	UE	UE+E	War-fare	Tur-moil	Riot-ing	Terrorism L.S.	Terrorism S.S.	Mu-tiny	Coup	Plots	Admin-Action	Quasi-Private
Aden	6	8	2		3	1				2		
Afghanistan	3	6			1	2				2		1
Albania [1]	6	12			4		2			1	5	
Algeria [2]	6	7	1		5					1		
Andorra												
Angola	1	1			1							
Argentina	35	57	1	1	10	1	16	5	1	13	8	1
Australia												
Austria	4	4			4							
Bechuanaland	3	3			3							
Belgium	8	8			1		7					
Bolivia	34	53			23	1	2	5	3	16	3	1
Brazil	36	49			27		4	2	2	5	7	
Br. Guiana	3	3			3							
Br. Honduras	1	1			1							
Br. No. Borneo												
Br. West Indies	1	1			1							
Bulgaria [1]	3	8			1	1		1			5	
Burma [2]	19	20	3		5	1	8	1	1		1	
Cameroons [2]	3	3	1		2							
Ceylon	9	10		1	6		2			1		
Chad	1	1			1							
Chile	9	21			8			1		6	6	
China [1]	18	25			14	3				3	4	
Colombia	42	47			30	3	8		2	5		
Congo	9	13			9							
(Middle) Congo R.	5	5			5							
Costa Rica	16	19	3		5		8			3		
Cuba [3]	80	100	1		26	2	48	2	1	13	7	4

INCIDENCE OF INTERNAL WARS, 1946-1959 (Continued)

	Totals		Unequivocal Cases (UE)						Equivocal Cases (E)			
	UE	UE+E	War-fare	Tur-moil	Riot-ing	Terrorism L.S.	Terrorism S.S.	Mu-tiny	Coup	Plots	Admin-Action	Quasi-Private
Cyprus	10	10			7	1	2			3	10	
Czech.[1]	11	24			5		5		1			
Denmark	1	1			1							
Dominican Rep.	2	6			1					2	2	
Ecuador	26	41		1	14	1	2	6	4	13	2	1
Egypt	30	45			20	1	6		2	11	3	
El Salvador	4	9			2				2	5		
Eritrea	4	5			2		2					1
Ethiopia	2	5			2					2		1
Finland	3	4			2		1			1		
Formosa		3								2	1	
France	40	46			33		6		1	4		1
Fr. Eq. Africa		1										
Fr. W. Africa	2	2			2						2	
Gabon												
Gambia												
West Germany	4	4			4							
East Germany	6	10		1	2		2	1				
Gold Coast (Ghana)	5	4		1	4					3	4	1
Gibraltar												
Great Britain	12	12			12							
Greece[2]	9	9	1		6		2					
Guadalupe												
Guatemala	32	45			12	7	10	2	1	8	5	
(Fr.) Guinea	1	1			1							
Haiti	32	40			12	3	13	1	4	5	3	
Hawaiian Is.												
Honduras	10	11			5		2			1		
Hong Kong	3	3			3				2			

INCIDENCE OF INTERNAL WARS, 1946-1959 (Continued)

	Totals		Unequivocal Cases (UE)			Terrorism				Equivocal Cases (E)		
	UE	UE+E	War-fare	Tur-moil	Riot-ing	L.S.	S.S.	Mu-tiny	Coup	Plots	Admin-Action	Quasi-Private
Hungary[1]	1	8		1							7	
India	79	83	2	3	55	11	6	2		1	1	2
(Fr.) India	3	3			2		1					
(Port.) India	7	8	3		3	1					1	
Indonesia[2]	24	24	5	1	7		3	8				
Iran	43	54			23	3	10	2	1	9	2	
Iraq	17	24			13	3	1			4	3	
Ireland[4]	8	9			4		3		1	1		
Israel	10	11				6	4			1		
Italy	48	51		4	19	1	24			2	1	
Japan	18	22		1	15		2				4	
Jordan	7	9			5		2			2		
Kenya[2]	8	8			6	1			1			
N. Korea	3	3					2	1				
S. Korea	22	39			12	2	7	1		8	9	
Lebanon	44	46			31	1	11	1		1		1
Liberia	1	1					1					
Libya	5	6			4		1					1
Luxembourg		1								1		
Malaya[2]	7	7	1		5				1			
Maldive Is.	2	2			1		1					
Malta	5	5			4		1					
Mexico	27	28		1	22		4			1		
Morocco[2]	19	20	1		16				2			1
Nepal	12	15	2		7	1	2			3		
Netherlands	2	2			2							
New Guinea	1	3			1					2		
Nicaragua[1]	13	16	1		2	1	7	1	1	1		2
Nigeria	8	8			7		1					

INCIDENCE OF INTERNAL WARS, 1946-1959 (Continued)

| | Totals | | Unequivocal Cases (UE) | | | | | | | Equivocal Cases (E) | | |
| | | | | | | Terrorism | | | | | | | |
	UE	UE+E	War-fare	Tur-moil	Riot-ing	L.S.	S.S.	Mu-tiny	Coup	Plots	Admin-Action	Quasi-Private
Norway	1	1					1					
Nyasaland	12	12			12							
Oman & Muscat	3	3	1				1	1				
Pakistan	14	18		1	11				2	4		
Panama	23	29			17	1	2		3	4	2	
Paraguay	19	29	2		6	1		3	7	9	1	
Peru	20	23			1	10	2	5	2	1		
Phillipines[2]	14	15	1	1	8	1	2	1		1		
Poland[1]	10	19			6		1	1			9	
Portugal	11	18			8		1	2		5	2	
Rhodesia & Nyasaland												
Ruanda Urundi	4	4			4							1
Rumania[1]	6	15			5				1		9	
San Marino	2	2			1							
Saudi Arabia												
Senegal	1	1			1							
Singapore	9	11			8		1				2	
Somalia												
Br. Somaliland												
Fr. Somaliland	1	1			1							
Spain[1]	22	22		1	5	1	15					
Sudan	10	10		5				3	2			
Surinam		1								1		
Sweden												
Switzerland												
Syria	20	26			14	1	5			4		2
Tanganyika	1	1			1							
Tangier	1	1			1							

INCIDENCE OF INTERNAL WARS, 1946-1959 (Continued)

	Totals		Unequivocal Cases (UE)							Equivocal Cases (E)		
						Terrorism						
	UE	UE+E	War-fare	Tur-moil	Riot-ing	L.S.	S.S.	Mu-tiny	Coup	Plots	Admin-Action	Quasi-Private
Thailand	11	20			3			3	5	7	2	
Togoland	1	2			1						1	
Tripolitania	3	3		1	2							
Tunisia²	9	9			6	2	1					
Turkey	8	13			6		2			4	1	
Uganda	4	4			3		1					
Union S. Africa	43	68			41		2				3	22
U. A. R.	1	1			1							
Uraguay		1								1		
Venezuela	26	36			15		2	5	4	8	2	
Yemen	1	2				1				1		
Yugosl. via¹	2	9	1				1				7	

VI

MILITARY DEVELOPMENT IN THE NEW COUNTRIES

Lucian W. Pye

During the past decade the newly emerging countries have come to impinge upon nearly every phase of American foreign policy-making. To a truly remarkable degree, the basic posture of the United States in world affairs has been colored by our sensitivity to the problems of the new countries. At times we seem to be acting as though we conceived the world struggle to be primarily over the sentiments and loyalties of the underdeveloped countries. In the sphere of military policy, however, the reaction to the problems posed by the new political status of the underdeveloped areas has been peculiarly ambivalent and indecisive. We have yet to achieve a clear appreciation of either the significance of military developments in the new countries of Asia and Africa or the place that such lands should occupy in American strategic planning.

It might seem that the revolutionary advances in military technology during the past decades would have destroyed any possible military significance of the underdeveloped areas. Even according to the conventional military calculus of a pre-nuclear age, countries with such low potential for organizing the instruments of violence would be insignificant. Yet in an odd fashion every major issue of defense policy during the last decade has entailed fundamental considerations of the underdeveloped areas. The doctrine of massive retaliation was primarily open to question because of the basic uncertainty as to whether it could be appropriately and convincingly applied to attacks on militarily impotent countries. Issues about appropriate delivery systems, particularly with respect to the importance of our advance base structure, again brought the underdeveloped areas to the forefront of military considerations. Questions about the choice of weapon systems revolve, in part, around the likely character of war in and among underdeveloped countries. Indeed, the fundamental issue of how it may be possible to utilize force in a rational fashion when thermonuclear arsenals exist has also been debated in terms of limited war in the underdeveloped areas.

We need not analyze here the numerous ways in which questions about military developments in the underdeveloped areas have been of persisting importance for American defense policy. To give emphasis to our

Born in China in 1921, Lucian W. Pye, PhD, is now Professor of Political Science and Senior Staff member of the Center of International Studies at the Massachusetts Institute of Technology. His research is mainly on political behavior in transitional societies, with particular emphasis upon Southeast Asia and China. He is a member of the Advisory Committee to the Director of the Agency for International Development and has lectured to all the War Colleges.

point we need only note that the underdeveloped areas have received central attention in our post-Korea alliances and in our military assistance programs.

Thus the paradox that in an age when the revolution in weapons technology has reduced the number of super-powers to only two, those countries which, according to all the conventional attributes of power, are of the least military significance have become the source of increasingly difficult problems. The American response has been one of seeking to eliminate the power vacuums which the new countries represent by building up their defense forces. It is not our purpose to detail the various phases of American policy with respect to military assistance to the underdeveloped areas except to note the vacillations and confusions of purpose which have informed these policies. Our initial adventures in military assistance were primarily in response to civil conflicts in which we sought to strengthen the anti-communist forces, as in our aid to Greece and Nationalist China. Subsequently, we became involved, as a part of our strategy of containment, in providing military reinforcements to weak countries adjacent to the communist bloc. After the Korean conflict, increasing amounts of military aid were directed toward providing internal security against guerilla and irregular forces. Military assistance has also been used as a diplomatic device to strengthen our political associations with underdeveloped countries.

These various approaches to military assistance to underdeveloped countries have been guided by different doctrinal considerations. In some situations we have seen military aid as a part of our general strategy for coping with the military threat of the communist powers. In other cases, our decisions have been governed by political calculations of a more limited nature, and in still other instances we have used military assistance as one means for stimulating domestic economic growth and social advancement in backward countries.

The fact that the impulse behind our numerous military assistance efforts has varied might have been of little significance had we not, through the accidents of legislative history, fallen into the practice of distinguishing between "military" and "economic" aid. In the early days of our foreign assistance efforts, Congressmen tended to favor "military" aid over "economic" aid because the former seemed to be more directly linked to our national interest. In more recent years economic development has been viewed as more acceptable and constructive by Congress. The combination of having to adjust to the moods of Congress and of lacking a consistent approach in all situations has seriously undermined the rationale for our military assistance effort in the underdeveloped areas.

In the light of these developments it would be helpful to try to clarify the relationship between "military" assistance and "economic" development before proceeding further with our analysis of military development in the underdeveloped areas. We will then turn to research needs, focusing our discussion primarily on the analysis of the conditions which make the military a positive or negative influence in the process of modernization. Finally, we will discuss United States policy questions which arise from the discussion as a whole.

MILITARY OR ECONOMIC AID

The manner in which political discussions about foreign aid in general have been carried out in recent years has led to a great deal of confusion over the purposes and practices of both military and economic aid. The logic which justified such a distinction in the first instance has been largely lost as advocates of limited programs have felt it politically expedient to suggest that there is a substantial difference between the two categories of aid.

Specifically, the champions of developmental aid on the basis of strictly economic criteria have challenged the validity and utility of military aid. which they tend to characterize as wasteful and, in their view even worse, a positive inhibition to effective economic growth. The charge has been made that American military aid has created abnormal situations in the recipient underdeveloped countries and has forced their governments to make grossly uneconomic allocations of their resources. Supposedly in these situations the military burden on the governments has been so great as to divert not only material resources and finances but also the attention and the energies of the government away from the essential, fundamental steps which must be taken to generate genuine economic growth. Those holding this position have tended to argue that in practice the concept of "defense support" was never effectively realized, for it has been impossible to provide for a government to maintain a stronger military establishment than its own resources would have made possible. It is contended that the existence of substantial military assistance diverts national leaders from performing the essential tasks necessary to set the stage for fundamental economic development.

Needless to say, most of the critics who employ considerations of the requirements for economic development in attacking military aid have felt little sense of urgency about possible military threats to the underdeveloped countries. As the example of the Korean invasion has faded in memory, the champions of more purely economic forms of development have increasingly contended that the communist threat in the underdeveloped areas is not military but arises almost entirely from domestic social and economic conditions.

These views, in combination with other considerations about the nature of internal wars in underdeveloped countries, have provided the basis for a gradually emerging American doctrine on counter-subversion. To this extent the advocates of military aid have been able to incorporate the considerations of their economic-development-minded critics while preserving the integrity of their prime military point of view. The military problems of maintaining law and order and of insuring civilian cooperation in case of war justify the expenditure of resources to reduce social and economic discontent. In theory, there could be no sharp point at which defense aid terminated and economic aid began. In practice, many projects, ranging from public health measures to the establishment of transportation and communications facilities, which are incorporated under "economic" aid in some countries are handled by "military" aid in others. There thus gradually developed considerable confusion in purpose between the two forms of aid and, to a considerable degree, the categories have lost their distinctive qualities.

Indeed, in pressing the case for the potentially constructive role military aid can have for economic development, some advocates have gone so far as to suggest that the military in many under-developed countries are more competent than civilians in performing certain crucial functions in furthering economic development. Thus, the goal of military aid becomes identical with that of economic aid and, instead of being a depressant to fundamental economic development, military aid can be justified as a most efficient way of stimulating national development. In the unstable setting of most underdeveloped countries, armies can best be expected to perform many of the crucial tasks essential for preparing the society for sustained economic growth.

Thus we see that during the few years in which American policy has been supporting military development in the underdeveloped areas there has been a substantial change in our public doctrines. The originally more popular category of military aid has given way to the greater faith in economic aid; but at the same time military aid has changed from being a possible obstacle to development into being a potentially positive factor in producing economic growth.

At this point we might observe the very constructive role that social science research might perform in helping to develop a sounder and more stable United States doctrine on foreign military assistance. There is enough historical evidence in records of our past efforts to make possible sober research into the matter of the degree which American military aid has either harmed or facilitated the national development of economically backward countries. It would seem urgently desirable to develop research on the basic question of how military aid might best be administered in order to facilitate, or at least not seriously retard, the economic development of a recipient country. Research could also give us a clearer understanding of both the potentialities and the limitations of the milit. ry establishments in new countries as agents in building up their nations.

THE MILITARY AND NATIONAL DEVELOPMENT

The realization that armies can play a positive role in economic development has come in part from recent historical developments. In eight Afro-Asian countries, armies had assumed control of their country's destiny by 1961, and in all cases they emphasized programs for economic and material advancement. An increased appreciation of the potentially positive role of armies in underdeveloped areas has also come from the recognition that many American programs of military aid have been engaged in precisely the same types of projects as covered by some of our programs of economic assistance. That is to say, the strongest arguments supporting the role of armies in economic development have been that the armies in underdeveloped countries are likely to behave more like civilian institutions than military establishments.

In the subsequent section we will have occasion to note the various reasons why military organizations are often more effective than civilian organizations in encouraging patterns of behavior which are conducive to national development. At this point we need only note that the increasing acceptance of an important role for armies in the developmental process

rests almost entirely upon the nonmilitary qualities of armies in the under-
developed societies. Among American economists the majority tend to feel
that the existence of large military establishments in most underdeveloped
areas would constitute a hindrance to national development. This attitude
reflects the traditional economic view that military establishments repre-
sent an uneconomic allocation of resources and that armies are never con-
structive institutions in any society. Thus, in much of the literature on the
problems of development, the military is looked upon as essentially a nec-
essary evil to be tolerated and accepted as a positive element only it is
adopts essentially civilian roles.

There is need for systematic research into the potentialities of mili-
tary establishments for guiding economic development and assisting in the
administration of national policies. It would be possible through such re-
search to determine various reasons why the military in different under-
developed countries has had varying degrees of success in furthering pol-
icies of economic development. There are, for example, serious problems
in the realm of organization which call for study: How can the military in
underdeveloped countries be effectively associated with the general planning
process so as to encourage a rational allocation of resources to national
development? To what extent can the military be effectively used to provide
administrative skills and guidance in the implementation of industrial and ·
agricultural projects? In posing such questions for research, it would be
important always to keep in mind the problem of whether such essentially
civilian roles are likely to compromise the military effectiveness of the
armies involved.

If the process of national development is viewed in broad terms and as
involving far more than just economic development, it becomes apparent
that the developmental function of the military can encompass far more
than just providing support for civilian economic developments. It would
seem in fact that the military has a most fundamental role to play in the
developmental process, a role which is related neither to its task of defend-
ing the nation from foreign attack nor to its ability to facilitate basic eco-
nomic and administrative developments. This role is essentially psych-
ological. It involves giving to a people a sense of identity and of national
pride.
One of the basic obstacles to development in most former colonial terri-
tories is the existence, particularly among the national leadership, of a
constellation of insecurities and inhibitions. The sense of inferiority and
the lack of assertiveness of a people who have once been dominated by for-
eigners cannot be easily eradicated. It has been frequently suggested--but
only by people who have never been subjugated--that the vigorous pursuit of
economic activities and the constructive spirit of economic development
programs can help a people overcome these feelings of insecurity and frus-
tration. The historical record, however, shows very few cases in which
success is essentially administrative and routinized tasks have provided
adequate compensations for earlier humiliations. People need more than
improvement in their economic life to find their basic sense of identity.

The need to achieve a sense of adequacy in the military realm seems to
be an essential prerequisite for national development. Indeed, competence
in the military arts has been one of the earliest indicators that a traditional
society may be advancing into the early stages of industrial development.

The two leading non-Western countries in successfully entering the modern
world are Japan and Turkey, and they have been the two countries most
effective in building their military establishments. It is therefore not
strange that India, one of the more successful of the currently developing
democratic countries in Asia, also has one of the most powerful and effi-
cient military establishments in Asia. The story of Chinese economic
and national development has been one of significant advances which follow
upon the initiative of developments in the military sphere. The relative
ranking of countries in Africa in terms of economic development and nation-
al cohesion coincides, in all except two cases, with widely shared judgements
of their relative military systems.

A very fundamental function of the military in the national develop-
mental process is to assist a people to gain a sense of self-respect and
dignity so that they can fulfill demanding and protracted community tasks.
In nearly all cultures, and certainly in the culture of the nation-state system,
manhood is closely associated with the warrior and the military arts. Mili-
tary development may thus be crucial in assisting former colonial peoples to
overcome their profound sense of inferiority. This is in part because any
people who feel that their national army is ludicrous and ineffectual must
also feel that their collective national identity is incapable of great things.*

At a more fundamental level, the military sphere appears to be a pe-
culiarly sensitive one psychologically because it touches upon the source
of national humiliation of former colonial people. Europeans may recognize
many areas in which they consider themselves to have been superior and
which provided the basis for colonialism; but almost all colonial peoples
are willing to admit their inferiority in only one area, the military. To
regain a sense of equality in their own eyes it thus becomes necessary for
these former subjugated peoples to feel that they have now redeemed them-
selves in the field of their initial greatest weakness.

The fact that a deep sense of military inferiority was a part of these
peoples' first reaction to the modern world seems in many cases to have
colored their capacity at present to modernize their societies. The lead-
ers often have profound psychological inhibitions toward making complete
and enthusiastic commitments to modernize their societies. In some cases
they are disturbed by the idea that they should try to emulate the ways of
their rulers who once insulted and mistreated their traditional culture.**
In many cases it seems that the best way, if not the only way, in which a
people can conquer these inhibitions and prepare themselves for the broad
tasks of national development, is for them to feel first of all that they have
been able to regain their self-respect in the military sphere. To make
full emotional commitment to modernization they must feel that they have
gained an equal footing with their former conquerors. Until such people
have gained a sense of national self-respect they are likely to feel that
their efforts at modernization serve only to remind them of their inferior-
ity relative to the European world.

_____

*For a more detailed treatment of the psychological ambivalences which
hinder national development in transitional societies, see Pye (19).

**For an insightful discussion of the deep psychological obstacles many
former colonial peoples have toward economic development and social
modernization, see Hagen (10).

The fact th t most of the colonial world has gained its independence without bloodshed or a test of arms makes this psychological problem an even more acute one. In countries where the "struggle for independence" was only a symbolic or verbal one there is often a peculiar need to establish as quickly as possible a respectable military force. This pattern has been the dominant one in all of independent Africa, and those few African countries which still lack their own symbols of organized force seem to resemble plantations run by administrative oligarchies which call themselves governments.

For the West even to appear to oppose military development in the new countries can be easily interpreted by the people, given these psychological considerations, to be an indication that the West persists in wanting to keep them in a subordinate and less than fully sovereign position. Rationally a very plausible case can be made for preventing the development of arms races in the African continent by strict controls over the supply of arms to separate African countries (see Rivkin, 20). Psychologically, however, such a policy would be most unrealistic and undesirable. Given the history which has gone before, it is often the case that a new country feels that the real test of when it has gained its full independence is passed when those who were its former rulers are willing to share with them the weapons and the means of violence which once were the monopoly of the European.

RESEARCH NEEDS

Broadly speaking, we have been suggesting that there are three points of view from which it would be most useful to consider the problems of politico-military development in the underdeveloped areas. First, there are the considerations relating to the world situation and American grand strategy. What in a military sense do the underdeveloped areas represent in the currently divided world? What kinds of war are most likely to be fought in these areas? And what types of military forces should we seek to encourage these countries to build up?

The second point of view sees the role of armies in the underdeveloped areas as a powerful source for assisting, and even guiding and stimulating, basic economic development. Under what conditions can armies further basic development, and what are the advantages of armies in being agents of modernization? The third approach concerns itself with the psychological role of armies in assisting a people in finding their sense of national identity and full autonomy.

Social science research should be able to make significant contributions with respect to all three of these approaches. To date, however, there has been relatively little work on the possible strategic roles of the military in the new countries. More research attention has been given to the problem of alliances and treaty commitments with underdeveloped countries. In recent years, for example, there has been a substantial literature on Southeast Asia Treaty Organization (SEATO) and the problems of military security in Southeast Asia.* Most of these studies have concentrated more

*See, for example, such studies as those by Fifield(9), Chatham House Study Group (5), King (15), Braibanti (3), and Birdwood (2).

on the political and diplomatic consequences of our military alliance systems. New programs of research on alliances are suggested by W. P. Davison's discussion on pages 31-42, above.

In the next few years it can be expected that there will be a growing interest among social scientists in the problems of guerrilla warfare and counter-subversion. It is, indeed, not impossible that this area may prove to be a more fruitful one for social scientists than many other aspects of military strategy. This is because the problems posed by such forms of warfare and violence are intimately related to questions about the social structure, culture, and behavior patterns of the populations involved in such conflicts. Without question, social science research is in a strong position to contribute useful knowledge in designing and developing internal security forces. Indeed most of our understanding about communist strategy and tactics in guerrilla warfare and subversion and of their basic appeals in underdeveloped countries has come out of the works of social scientists.*

Harry Eckstein discusses the conditions which lead to internal warfare in Chapter V, above. Hence, we need only conclude with the observation that the task of developing free-world techniques for coping with subversives and guerrilla warfare will call for substantial research efforts. The problem cannot be dealt with according to the conventional uses of military forces. This suggests a natural area for cooperation among military specialists, social scientists, and creative engineers of new weapon systems.

With respect to our second approach which deals with the role of the military in the economic and political development of new countries, there are again great possibilities for some exceedingly disciplined and systematic research. In the last few years social scientists have become increasingly interested in the political and administrative roles which armies can play in the nation building process. The RAND Corporation, for example, held a conference on this subject in 1960 (Johnson, 13). More detailed work has done on the role of the military in particular regions or in particular countries.** There is in addition a long tradition of historical analyses of the domestic political role of military leaders, but unfortunately most of the work in the field of civil-military relations in the West has only marginal application for understanding current problems in the newly-emergent nations.

On the basis of this record of research it would seem that the stage is now set for rigorous studies which might inform us of the conditions under which military establishments in the new countries are most likely to act in ways favorable to national development. This would be research which could be of great value for policy-makers, for it would be directed to the answering of precisely the same kinds of questions as often trouble those responsible for policy.

The third of our approaches, which involves the psychological function of the military in providing a stronger sense of national self-confidence,

---

*See, for example, such studies as those by Brimwell (4), Kennedy (14), Fall (7 and 8), Scaff (22), Hanrahan (11), and Pye (18).
**See, for example: Pauker (17), Vatikiotis (25), Rustow (21), and Berger (1).

has received the least amount of attention from social scientists. It would
however. seem possible for social psychologists to analyze the extent to
which it is possible to find functional equivalents to war as a means for giv-
ing a people a sense of national identity and self-respect. It would be of
great value to have studies made of how people in transitional societies at
various stages of development tend to perceive and emotionally and intel-
lectually respond to their nation's military forces. We need to know more
about the extent to which the army may be a fundamental institution in pro-
viding national pride and a national political consensus.

It is a disturbing fact that in spite of a decade of massive involvement
in extending military aid to underdeveloped countries we have not built up a
body of sound knowledge about military development from any one of these
three points of view. American experience in cooperating in the building
up of new military forces has not been adequately codified, and we are still
continuing to act without the benefits of detailed and systematic research.
Most of the current literature on military aid consists of various efforts
to justify or to attack the basic concept of military assistance programs.*
There has been very little substantive research on the problems of military
development themselves.

In the light of this great shortage in research it seems appropriate to
organize the remainder of this study around different areas of potentially
fruitful research. The fundamental questions to which we shall address
ourselves in the following discussion will be: What are the conditions
which will make the military develop into a positive force for modernization?
What conditions will produce a military which will impede such development
and become a reactionary element? These questions will be discussed in
terms of (a) historical analyses. (b) the contributions of armies to prep-
aration for the modern world, and (c) the governmental potentials of armies.

Historical Analyses

There is a substantial body of historical literature which deals with
the political roles that military commanders have played in national devel-
opment. Aside from recognizing the coercive power at the command of
such leaders, most of these studies tend to ignore the nature of military
institutions. Such studies have failed to deal with the organic relations be-
tween military development and national development in a society.

It would seem that the roles that the military has been able to play have
been determined, first, by the state of competency of other ins tutions in
the society; second, by the internal conditions of the military; and third, by
the state of civil-military relations defined in the broadest terms.

By and large, it would seem that institutions within a society tend to
develop at approximately the same pace, and if any disequilibrium develops
it will generally produce significant tensions' capable of altering the constit-
utional bases of the policy. Thus in general the capacity of society's gov-
ernmental administration to manage its public affairs, its economic insti-
tutions to allocate resources, its political process to relate power to
values, and its military establishment to command violence should all

---

*The best studies of military aid include: Wolf (26), Draper Commit-
tee Report (6), U.S. Senate publications (23 and 24), and Nitze (16).

develop approximately in parallel. The problem for research which is of prime interest to us is the probable consequences for national development whenever a significant differential emerges between military development and the development of the other institutions of the society.

Historically it would seem clear that there is a considerable difference be'ween the state of affairs which occurs (a) when the military emerges as a powerful force because it was built up to meet the problems of war and (b) when the military appears to be the most effective organization in the society because civilian institutions have failed in performing their basic governmental functions. With respect to the first alternative, it also seems to be important whether the military build-up was relat..d to a successful or a disastrous war.

It is therefore significant to note that the Ataturk model of national development and social modernization, which has often been cited as possibly deserving of emulation, emerged out of a postwar situation in a defeated but not seriously damaged country. Turkish society, as a result of its involvement in World War I, had already experienced the political focusing of all life which generally accompanies war, and most of its politically attentive public had been sensitized to issues of national honor and national effort. Turkey, after its defeat and its loss of non-Turkish empire, was far more aware of its own sense of national identity in the postwar period than it had been under the Ottomans just before World War I. This was quite a different situation from that commonly found in the newer Afro-Asian states which have come under military rule. In these newer states, the society has not been effectively mobilized, and the military has been either filling a near vacuum or bringing a degree of order out of the political chaos created by civilian politicians. In these cases the military may appear to be powerful and effective and thus in a potential position to play an Ataturk role, but in fact it may lack the means of mobilizing a public which has not been previously trained to respond to national issues.

In most of the new countries in which armies have come to power, the build-up of the military has not been related to international wars and a process of national mobilization. In Pakistan, foreign military aid and the threat of international conflict produced an inordinately high investment in the military establishment; in Burma and Indonesia, chronic internal disorder and insurrections resulted in the military's requiring an inordinately high proportion of the nation's resources. In these situations armies have found themselves in favored positions as compared with civilian authorities, and on the basis of their more adequate resources, armies have generally been able to call for more effective actions in a wider range of fields than any other single institution in the society. On the other hand, since the process of military build-up was not related to a popular war effort, armies which gain dominance under such conditions usually do not have effective ties with the masses of the population. Lacking the political and the psycho-ideological relationships which can develop between the military and the civilian mass during wartime, such armies may find that their supremacy over civilian institutions does not carry with it the capacity to gain the popular support needed for effectively ruling a country and initiating new programs for national development.

Thus, for example, the armies in both Burma and Indonesia were able to make many reforms with respect to the civilian administrative structure,

but they found that they lacked the necessary means for mobilizing popular
support for their proposals for reform. In Indonesia the army has grad-
ually accepted the need for politicians to influence mass behavior. In
Burma the army had to give up whatever ambitions it had for direct rule
because it could no longer maintain its essential character as an army while
seeking also to perform the functions of the politicians.

A survey of the historical examples of effective military encouragement
of national development suggests that an ideal condition for such a relation-
ship is the state of affairs shortly after the country has been defeated or
h· miliated but the army has not been seriously damaged, and an easy
scapegoat exists in the form of a discredited civilian elite. These were
the conditions which surrounded the rise of Ataturk in Turkey and of, first,
Chiang Kai-shek and, then, the Communists in China. If, on the other hand,
the army must recognize that defeat was attributable to its own inherent
weaknesses, it may so sense its frustrations as to fail in asserting effective
national leadership. In this case it is useful to compare the Nasser who led
the Egyptian revolution against the Farouk government, after the first defeat
of the Egyptian army at the hands of Israel, with the Nasser of the post-
Suez period.

Victorious armies that are aware that they only narrowly averted defeat
may also adopt a vigorous approach toward national development. For ex-
ample, the Japanese army, although successful in its expedition in Formosa
in 1874, realized from the experience that a considerably greater national
effort was necessary if the country was to meet any more serious inter-
national tests. Indeed, the combination of initial success and continued
awareness of inadequacies against the next potential foes was the basic
theme throughout the history of the influence of the Japanese army on the
development of modern Japan.

In summary, it may be said that there is a need for historical research
which can produce a richer array of hypothesis about the potential relation-
ships between military development and nation building. There appear to
be only certain conditions under which the military can take the decisive
lead in producing effective development. Other conditions seem to call for
a much more limited role for the military. On the basis of such research,
it may appear that even with substantial outside assistance certain armies
may still not be in a position to perform the Ataturk role; and therefore
American policy, for example, might more appropriately seek limited bene-
fits from military development in many underdeveloped countries.

Armies and Preparation for the Modern World

There is considerable need for further research on the more limited
but still potentially extremely important role that armies can play in pro-
viding new skills, attitudes, and values for the modernization of transitional
societies. Already, and without benefit of explicitly designed policies
for the purpose, armies in many underdeveloped countries are performing
vital economic and sociological functions in bringing peasant-oriented peo-
ple into a more technologically advanced world. In most of the countries
of Asia, military experience has been a prime means of training people in
the operation and maintenance of motor vehicles. In all industrially ad-
vanced societies, the military establishments have become important in-
stitutions, for subsidizing the production of a host of technical skills for the

national economy. In transitional societies where there is a great absence
of alternative training institutions, armies can play an even more signifi-
cant role in strengthening the technological basis of the society.

Military training in such societies can mean far more than merely
introducing large numbers of people to more advanced machines. Induction
into military life can also be one of the most economic and rational ways
of inducting tradition-bound people into the environment of modern organ-
izational life. In addition to learning new skills and obtaining new infor-
mation, the new recruit is exposed to all the constellations of attitudes and
sentiments which are the functional prerequisites of the more complex, im-
personal, and standardized patterns of behavior necessary for the support
of modern social, economic, and political institutions.

Many Asians and Africans have been taken from the known social envi-
ronment of their villages and tribes and thrust into the unknown atmosphere
of urban life. This process has generally produced a high degree of inse-
curity and a need to adopt new institutions in the urban setting. The process
has been fundamentally disruptive to the general social and economic deve-
lopment of modern institutions. In some cases the insecurities of city life
have encouraged the new migrant to fall back upon class and regional asso-
ciations which were known in the traditional society but which are now
given new and much stronger functions. Such associations, however, may
inhibit the effective integration of the new community. Specific examples
of these dysfunctional activities are the strengthening of "provincial" and
"benevolent" associations among newly urbanized overseas Chinese and the
strengthening of some aspects of caste among certain groups in urban India.

In other cultures the unorganized breaking from tradition has resulted
in the creation of a host of new urban associations, designed either to ex-
ploit the new and unsure city dweller or to provide him with socially unde-
sirable modes of escapism. It is hard to imagine how economic development
and stable politics can be built out of the sense of anomie which charac-
terizes urban life in some African countries.

In many transitional societies it is apparent that the army does consti-
tute a vehicle for bringing people into modern life with a minimum of social
and psychological strain. Recruits are expected to change their ways of
life and their habits of thought, and the process of becoming a modernized
soldier is not too dissimilar to that of becoming a useful citizen in a mod-
ernizing society. Army life can give the individual a basic sense of se-
curity as he adopts new loyalties and learns that impersonal relationships
need not be dangerous and threatening ones.

Finally it should be noted that military development can be a powerful
instrument for producing a politically loyal citizenry. In new countries the
military must concern itself with instilling in its troops a sense of loyalty
to the completely new concept of the nation. In such societies there are
relatively few politically non-partisan institutions devoted to strengthening
a popular sense of national identity based upon personal obligations and
sacrifice. Indeed, in most non-communist underdeveloped countries there
are extremely few leadership groups willing to call for the high degree of
personal sacrifice which is essential for economic development and national
advancement. Political leaders prefer to suggest that development can be
readily realized simply by the manipulation of symbols, the pronouncement

of programs, and the initiation of plans. It is therefore extremely imp-
ortant to recognize that armies in transitional societies may have oblig-
ations in the political sphere which extend far beyond those assumed by
armies in more developed and politically stable societies.

At present we do not have the necessary information to explain under
what conditions soldiers are likely to revert back to their traditional cul-
tures once they leave the army and under what conditions they are likely
to carry new sets of attitudes with them back into civilian life. It seems
this would be a significant area in which social science research might help
policy planning. It seems also that the character of the army training ex-
perience would be a crucial factor in influencing the intensity and the depth
of attitudinal changes. And the general attitudes of the public toward the
military are likely to weigh heavily in determining the extent to which the
learning experience in the army can become a permanent one.

The concept of a politically neutral military is basic to democratic
development, and this is as true in underdeveloped as in industrial socie-
ties. On the other hand, it must be recognized that as long as basic con-
stitutional issues have not been resolved in transitional societies it will be
impossible for the military to perform its part in a politically neutral sense.

In some situations, it is clearly apparent that the military must assume
a more direct political role if democratic development is to occur. The
important point is that analysis can suggest when the intervention of the
military into the political sphere is likely to result in authoritarian patterns and
when such intervention may serve to strengthen inherently democratic
developments.

At this stage of social science research it is possible to point to many
very significant roles which armies can play in advancing economic and
political development. At the same time there has not been enough research
to inform us of either the most efficient ways of exploiting the multi-func-
tional character of armies or of the real dangers of demanding too many
tasks of an army. Clearly, in poor countries it makes sense to use armies
to perform a host of other non-military functions, but we need to know more
about what the military costs may be of such diversified use of armies. It
is clear that not all attempts to give armies non-military functions will tax
their military effectiveness, as can be seen from both the military and the
non-military potentialities of the Israeli Army. At the same time it would
be dangerous to oversell to underdeveloped countries the modernizing po-
tentials of armies.

One area in which social science research might be of immediate assist-
ance is in helping to devise appropriate educational programs for the per-
sonnel in the armies of the new countries. In recent years the United States
Army has developed an impressive array of specialist schools and has sig-
nificantly advanced the techniques of educating and training in a non-acad-
emic environment. Allied military personnel have been sent to many of
these schools, with remarkably good results. However, the knowledge does
not always exist for appropriate adaptation of educational programs de-
signed for United States officers and troops to those from the new countries.
The situation clearly calls for extensive study of the most appropriate
educational and training programs for the military in the rapidly changing
societies. The objective of such education should be to transmit the skills

and attitudes necessary for modernizing the country. There is also the
need for educating the officer class to its civic responsibilities to its
country. Through such education it may be possible to reduce the threat
of the military's becoming an obstacle to national de/elopment and pro-
gress.

Governmental Potentials of Armies

It is at the point where the actions of the military begin to overlap with
the exercise of civil, governmental authorities that we come to the most
delicate relationship between military development and the creation of new
societies in the underdeveloped world. Historically, the military has cust-
oma ily been an arm of the civil authority, and it usually represents one of
the important elements in the authoritative structure of government. The
ba ce between the military and civilian authorities can be a most crucial
question determining the entire pattern of national development.

The basic problem in most underdeveloped societies is the difficulty
in creating effective organizations capable of sustaining all the activities
basic to modern life. There generally is an imbalance in the development
of organizations with the result that the more effectively organized groups
are called upon to perform functions generally associated with other types
of organization. Another feature of the developmental process is that the
more authoritative organizations are usually more easily established than
the more complex and diversified, especially those dealing with decision-
making.

This is to say that there is generally a high degree of substitutability
of roles in transitional societies, and the more concrete and authoritarian
organizations tend to assume the duties of the less explicitly structured
organizations. The development of the complex, market-oriented economy
usually lags behind the establishment of bureaucratic authorities, and gov-
ernments tend to utilize explicit controls rather than rely upon market mech-
anisms for economic decision-making. Similarly, the military authorities
often find th t they are in control of one of the most effective general pur-
pose organizations in the society, and hence they may be called upon, or
be compelled by events, to perform the duties of civil authorities.

This is a clearly established pattern in the process of nation building.
We tend today to forget the role performed by the United States Army, first,
in establishing law and order in the West and, then, through the Corps of
Engineers, in building what the economists call the necessary social over-
head capital for economic development. In all societies, the military is by
force of necessity a crucial factor in influencing the process of social re-
organization and community development. Current notions of appropriate
civil-military relations in developed countries tend to overlook what have
always been the broad functions of the military in establishing the infra-
structure of the modern nation-state.

A basic question is that of determining the types of military interven-
tion which may have long-run negative effects on national development and
democratic modernization. It is not at all clear that restraint on the part
of the military in assuming civilian functions is necessarily the correct
approach. On the contrary, effective national development may call for
armies in industrial societies. The underdeveloped societies should not

be deprived of the developmental value of military organizations simply because the ideology in advanced societies rejects the assumptions of civilian functions.

If the developmental process is to be assisted, it is essential for us to learn more about how armies can assume wider functions without eventually disrupting or impeding the developmental process. In countries faced with serious insurgency or subversion, it may be essential for the military to assume many civil affairs functions and even to operate as the prime institution of government. The problem is not only how well the military can perform such functions but also whether they can perform them in such a manner as not to obstruct the basic development of the society and the eventual strengthening of civilian institutions.

The peculiar advantages of explicit lines of authority and of a professional tradition of service do assist the military in becoming an effective organization in otherwise disorganized societies. This inherent advantage thus forces the military into a tutelage role, and unfortunately there is no issue with respect to nation building which is as intellectually challenging or politically difficult as that of the degree and nature of tutelage necessary for effective development. To what extent can and should a particular elite justify its new monopoly of decision-making powers on the grounds that the rest of the society is not capable of effective organized actions?

When the problem is posed in these terms it becomes apparent that the military can easily find itself in much the same position as colonial authorities and the leaders of single party nationalist movements. The art of devolution of power is extremely difficult to master, and those who are instructing a society in the ways of democracy may be extremely reluctant to give up the powers of being the teacher. In some respects the termination of colonial rule is a simpler problem than that of ending the political tutelage functions of an indigenous army. At some point the colonial authorities must leave the land and thus decisively end their influence, but armies that only go back to the barracks can always return to the field of government on short notice.

A strategy for the use of military institutions in nation building must include some consideration of how the tutelage functions might best be terminated. It would seem that the most successful strategy would call for a gradual evolution of military functions so that certain primary administrative responsibilities might revert to civilian hands while the army continues to contribute to national development by providing skills and knowledge at ever higher levels of technology. In Western societies the military plays a crucial role in advancing some of the most sophisticated technological developments in the society. Research might assist the military in transitional societies to shift gradually to a similar role.

In order to evaluate more fully the potentialities of armies in providing political tutelage, it is necessary to have further research into the social composition of the officer classes in transitional societies. There is considerable evidence that the changing political role of armies in such countries is often related to a significant change in the social and economic backgrounds of the officers. In particular, in those transitional societies in which there has been a decided shift in the political role of the army from

a more conservative and status-oriented position to becoming a dynamic
force for development, there has generally been a change in the recruitment
of the officers from the established, landed families to more urban and more
middle-class families. This pattern of change has been observed in both
Latin America and the Middle East. If the experience of the United States
and European armies is a guide, such a pattern of change will be facilitated
by an increasingly technological orientation of the military (cf. Janowitz,
12).

On the basis of research into the social composition of the officer
class of various armies it should be possible to forecast the degree to which
particular armies are likely to take a leading role in guiding national devel-
opment. It should be noted that, whenever the social positions of the mili-
tary are seen as being in excessively sharp conflict with the leading civil-
ian groups, there can be tensions which may not retard national development
but may produce many anti-democratic effects. For example, the Japanese
Army before World War II was generally recruited from lower and more
rural classes than were the bureaucratic and political leaders of the coun-
try. Consequently, to some degree the clash between the military and the
civilian authorities was the playing out of social conflicts. Specifically,
the Japanese military were suspicious of the civilian leaders, doubting
whether they were fully committed to the spirit of Japanese nationalism.
Thus, although the Japanese military pushed for national development, it
was almost entirely on terms which heightened the possibility of interna-
tional tension and reduced the likelihood of balanced and stable national
growth.

UNITED STATES POLICY PROBLEMS

Although in our analysis we have stressed the potential the military
has for advancing the economic and political development of the new coun-
tries, we should not overlook the obvious fact that the military in many un-
derdeveloped areas has been one of the most powerful forces retarding
modernization. This has been particularly the case when the officer class
has been closely related to a feudal, landed aristocracy. In much of Latin
America the history has been one in which the military has impeded democ-
ratic and economic development.

As noted above, however, in recent years there have been rapid advances
in military technology, and it has become increasingly difficult for armies to
remain as isolated, closed societies which stress status and caste consid-
erations. The art of warfare now calls for more and more of the skills and
attributes basic to an industrialized society.

These considerations suggest that, as United States policy pushes for
the military strengthening of the armies of certain underdeveloped countries,
it is also interjecting into these societies a greater respect for the skills
and attitudes essential for modern society. In order to take full advantage
of the opportunity, we must insure that the military does not become a
force of reaction.

The very real danger of the military's becoming an obstacle to social
change suggests the need for developing techniques to impress upon the
military leaders of the new countries a strong sense of responsibility and

of pride in profession. In numerous ways the American military is in a
position to help create the appropriate sets of attitudes and values in our
allied military leaders. Through the associations which are established
by our Military Advisory Groups, and the many kinds of schools at which
allied officers study, we have the bases for communications about the con-
tributions and the limitations of armies in building nations. Indeed, there
is no other group of people in the new countries with whom we have more
associations at an official level than the military.

If we are to realize the potentialities that the military establishments
represent in the new countries and the opportunities of our favorable basis
of communications with them, we must establish a clearer understanding
of the processes of political development and of what can and should be done
to advance the transition of backward societies. This situation calls for
the formulation of a doctrine for political development which can serve
as a guide to all manner of policy efforts in the new countries.

Over the last few years we have as a nation been approaching the prob-
lem from different, isolated sectors. Thus those who are responsible for
economic aid have seen the problem of development mainly from the nar-
row perspective of actions designed merely to affect the economic system,
or even isolated aspects of the economy, and they have not, quite naturally,
been centrally concerned with the total process of social and political de-
velopment. The same can be said of those responsible for cultural and
educational assistance programs. Military assistance has also approached
the problem from a single sphere of activities. We have increasingly come
to realize that national development in transitional societies cannot be
achieved by piecemeal approaches and that the sum of all the separate ap-
proaches does not add up to genuine development. As a consequence, there
has been a growing interest in the formulation of more comprehensive and
more coherent national programs of development. The concept of a "coun-
try team" organization under the leadership of the ambassador reflects
this rising concern for an overall national approach to development.

With the emergence of a general concept of national development, it is
important that the full potentialities of the role of the military not be
slighted. There is a danger, given the historic Western feelings about a
rigorous division between civil and military spheres, that those involved
in building such a doctrine for American policy will tend to neglect the full
role that the military can play in the nation-building process. As we have
suggested repeatedly in this report, it is quite clear that social science
research is in a position to reveal many different ways in which the mili-
tary in transitional societies can make a positive contribution to national
development.

Possibly an even more important reason why the role of the military
should be carefully and explicitly treated in any doctrine of national devel-
opment is the tremendous harm that armies can cause to a nation's growth.
We have tended in this study to stress the positive potential of armies, but
in doing so we have been highly conscious of how frequently the military
has constituted a major obstacle to all forms of social, economic, and po-
litical progress. Historically, more often than not, poor countries have
suffered from the effects of the unenlightened political policies and ambi-
tions of their military forces. It is precisely because there is such a
great danger that the military will retard national development that we

have been so interested in discovering whatever positive potentialities armies may have in facilitating economic and political advancement. The burden which incompetent and backward-looking military leaders have imposed upon their country may indeed be one of the greatest obstacles to progress in the entire history of the underdeveloped countries.

With these considerations in mind it may not be an exaggeration to suggest that one of the greatest contributions which social science research might contribute to the advancement of the underdeveloped areas may be in the field of learning more about how armies can facilitate and not impede development. An awareness of the great dangers that armies can be to national development should strengthen our feeling of responsibility for insuring that our assistance to the military of the new countries will advance their national wellbeing.

REFERENCES

1. Berger, Morroe. The Military Elite and Social Change: Egypt Since Napoleon. Princeton, N.J.: Princeton University, Center of International Studies, 1960.

2. Birdwood, Lord Christopher B. "The Defence of South East Asia," International Affairs, 31 (1955), 17-25.

3. Braibanti, Ralph. "The Southeast Asia Collective Defense Treaty," Pacific Affairs, 30 (1957), 321-341.

4. Brimwell, J. H. Communism in Southeast Asia. London: Oxford University Press, 1959.

5. Chatham House study group. Collective Defence in South East Asia: The Manila Treaty and Its Implications. London: Royal Institute of International Affairs, 1956.

6. "Draper Report." Composite Report of The President's Committee To Study the United States Military Assistance Program. Vols. I and II. Washington, D. C.: Government Pringing Office, 1959.

7. Fall, Bernard. The Street Without Joy. Harrisburg, Pa.: The Stackpole Co., 1961.

8. _____ . The Viet Minh Regime. Ithaca, N. Y.: Cornell University Press, 1954.

9. Fifield, Russell H. The Diplomacy of Southeast Asia. New York: Harper and Bros., 1958.

10. Hagen, Everett E. On the Theory of Social Change. New York: Richard S. Irwin, 1962.

11. Hanrahan, Gene. The Communist Struggle in Malaya. New York: Institute of Pacific Relations, 1954.

REFERENCES (Continued)

12. Janowitz, Morris. The Professional Soldier: A Social and Political Portrait. Glencoe, Ill.: The Free Press, 1960.

13. Johnson, John (ed.). The Role of the Military in Underdeveloped Countries. Princeton, N.J.: Princeton University Press, 1962.

14. Kennedy, Malcolm. A History of Communism in East Asia. New York: Frederick A. Praeger, 1957.

15. King, John Kerry. Southeast Asia in Perspective. New York: Macmillan Co., 1956.

16. Nitze, Paul H. "The Policy Background for Military Assistance," in International Stability and Progress. (Pp. 99-119.) New York: Columbia University, The American Assembly, 1957.

17. Pauker, Guy J. "Southeast Asia as a Problem Area in the Next Decade." World Politics, 11 (1959), 325-345.

18. Pye, Lucian W. Guerrilla Communism in Malaya. Princeton, N.J.: Princeton University Press, 1956.

19. _____. Politics, Personality, and Nation Building. New Haven, Conn.: Yale University Press, 1962.

20. Rivkin, Arnold. "Arms for Africa?" Foreign Affairs, 38 (1959), 84-94.

21. Rustow, Dankwart A. "The Army and the Founding of the Turkish Republic." World Politics, 11 (1959), 513-552.

22. Scaff, Alvin H. The Philippine Answer to Communism. Palo Alto, Calif.: Stanford University Press, 1955.

23. Senate, U.S. Military Assistance Program, Joint Hearings Before the Committee on Foreign Relations and the Committee on Armed Services. U.S. Senate. Washington, D.C.: Government Printing Office, 1949.

24. _____. The Military Assistance Program of the United States, Two Studies and a Report Prepared at the Request of the Special Committee to Study the Foreign Aid Program. U.S. Senate, Committee Print. Washington, D.C.: Government Printing Office, 1957.

25. Vatikiotis, P.J. The Egyptian Army in Politics. Bloomington: University of Indiana Press, 1961.

26. Wolf, Charles, Jr. Foreign Aid: Theory and Practice in Southern Asia. Princeton, N.J.: Princeton University Press, 1960.

VII

POPULATION RESEARCH AND THE NATIONAL INTEREST

Ansley J. Coale

The scientific study of population and the careful use of population data have contributed much, and can contribute more, to an understanding of military problems and the intelligent management of military affairs. Two examples of valuable demographic analysis are: (a) the use of current data about the population of the United States (and projections of the U.S. population) in military planning, including plans to man the armed services, and (b) the use of foreign population data and estimates as an important component of military intelligence. However, because of special features of the current nature and position of population research, a canvass of the possibilities of its more extensive and effective direct contributions to the problems of the military establishment would not indicate how demographic research might best serve the national interest, or national security. I shall argue that neither a large expansion of population research within the military establishment nor immediate Defense Department support for research on demographic military problems deserves the highest priority. More immediate needs are for an expansion in the number of expert demographers (through more widespread graduate training) and for expanded basic research on human populations.

A balanced view of the potential contributions demography might make to the national interest should take account of the following considerations:

a. Mere size of population appears not to be the major determinant of military strength in this era of advanced military technology and great international inequalities in technical advancement and industrial capacity. As a consequence, demographic research of direct usefulness to the Department of Defense, while indispensable, is somewhat limited in scope. Directly useful demographic projects (such as foreign manpower analysis) can continue to be conducted effectively as in the past by special demographic research units within the government. The use of classified information (derived, for example, from intelligence reports) makes government agencies, or agencies that specialize in problems of national security such as RAND, the natural loci of such research. Demographic projects, specifically designed to be useful to the military, need a modest additional number of well qualified population experts as staff and consultants.

Ansley J. Coale, PhD, is Director of the Office of Population Research and Professor of Economics at Princeton University. He is a Consultant to the President's Scientific Advisory Committee and the RAND Corporation and has been a Consultant to the Weapons Systems Advisory Group and the Bureau of the Census. In 1962, Dr. Coale was U.S. Representative to the Population Commission of the United Nations. He is a member of the American Philosophical Society as well as of learned societies in the fields of population, economics, and statistics.

But a more profound need is a greater fund of basic scientific knowledge and technique for such projects to draw on.

b. There are broad issues of national security in which population plays a larger part than it does in the everyday problems of the military establishment. Some of these issues are domestic, an example being the influence of population on the pace and form of future American economic growth, but the more pressing issues in which population is a major element are international in scope. Perhaps the most important of these are the precarious economic future of the underdeveloped areas, the instability of some of their governments, and the constant pressure to extend communist influence among the underdeveloped countries. Population trends will play a key role in determining economic progress in the low-income countries. The future of these countries, containing as they do more than half of the world's people, is a matter of concern to the United States and its government for reasons of simple humanity. It also concerns us because in this era of political ferment, internal war, and communist penetration, our security is linked to the well-being and stability of the low-income countries.

c. Most underdeveloped countries are nearly devoid of persons with the training needed to conduct censuses, to record births and deaths, and generally to provide the data and analysis required to administer health, education, industrial development and other vital programs that depend on information about population. In other words, many of these countries are at present ill-equipped to understand or to measure their population problems, much less to formulate policies to cope with them. This shortage of basic skill can be alleviated only by accelerated training in demography-- training that must be conducted for many years in good part by experts from advanced countries such as the United States.

d. Much of the basic knowledge needed for an effective confrontation of the population problems of the rapidly growing low-income countries is not yet part of the inventory of demographic science. Demographers have made substantial discoveries in recent years about the interrelations between birth rates, death rates, and the size, growth, and age distribution of populations, on the one hand, and the social and economic changes that characterize the modernization of pre-industrial countries, on the other. But knowledge remains woefully inadequate. Work on these basic problems deserves a high priority on the agenda for population research.

e. The greatest barrier to more effective basic and applied research on population is the scarcity of qualified experts rather than any lack of financial support for worthwhile projects. In recent years most well designed projects have been able to obtain financing from private foundations, or, in some instances, from the National Science Foundation or other government sources. But research within the government and in the universities, the teaching of undergraduate and graduate students and the training of demographers from abroad, and the conduct of large-scale demographic statistical operations in various government departments, federal and state--all are manned from a very meager suppy of trained demographers. The professional society in population--the Population Association of America--has between 500 and 600 members in contrast to the more than 19,000 members of the American Psychological Association, 7,000 in the Political Science Association, 7,000 in the Statistical Association, and 10,000 in the American

Economic Association. I do not mean to suggest that it would be possible or
at all desirable to have as many demographers as there are psychologists,
political scientists, statisticians, or economists. But, so long as there re-
mains only a very small number of trained demographers, financial sup-
port for research on particular population problems may simply divert
effort from problems of at least equal ultimate importance. No great in-
crease in the effective contribution of population research to the national
interest is likely to occur without an increase in the number of good demog-
raphers and without the balanced development of basic knowledge in the
field, both of which are more than adequately justified by the increasing
gravity of population problems.

In the remainder of this essay I first discuss the influence of popu-
lation on potential military strength and then turn to the broader popula-
tion problems that are likely to affect (directly or indirectly) the national
security of the United States. Finally, I evaluate the importance of ex-
panding, first, graduate training in demography and, then, the volume of
basic research.

POPULATION AND MILITARY POTENTIAL

The relative military capacity of countries in the twentieth century is
strongly dependent on the state of technology they have achieved and on their
capacity for industrial output. Population, it appears, is a major determi-
nant of relative strength only among countries having access to roughly
equivalent technologies.

A preliminary division of countries into categories of advanced and
industrialized, on the one hand, and technical and industrially underdevel-
oped, on the other, will make it easier to appreciate the role of population
per se. The countries in the first category typically have: a wide diversity
of occupations with only a minority of the labor force engaged in agricul-
ture; a high level of specialization with most goods and services being ob-
tained through monetary transactions in organized markets rather than ex-
changed by barter or produced and consumed within households; elaborate
systems of communication and transportation; a large endowment of cap-
ital equipment in the form of modern power-driven machines and nearly
universal employment of electricity in households and factories; mainly
highly trained persons engaged in scientific and technological research;
small proportions of the population illiterate, nearly universal primary
education, widespread secondary education, and a well-developed system
of universities; and high per capita income. The technically and indust-
rially underdeveloped countries have opposite characteristics; i.e., less
diversified occupations with a majority of the labor force in agriculture;
less specialization with the family or household serving as the typical unit
of production; etc.

For the past century or more, the industrially advanced countries have
had a military advantage over the underdeveloped countries, generally

transcending any differences in population.* In fact, a more advanced country can sometimes put more men in combat against an adversary with a much larger base population. In 1894, Japan's population was no more than one-eigth that of China, yet she was able to field an army which, in addition to other advantages, had more men than the Chinese (see Renouvin, il).

Strength in nuclear warfare is even more closely associated with industrial capacity and the state of technology (and less closely with population) than is conventional military strength. Sixteen years after the first test explosion, nuclear explosives have been constructed solely by four great industrial powers. If no international limitation on manufacture is achieved, less advanced countries may come to produce nuclear weapons, as simple and cheaper methods of manufacturing fissionable material are discovered and as nuclear engineering diffuses through the world. But for a few decades, at least, only the wealthiest industrial nations will be able to construct a large stockpile of weapons and effective delivery systems as well.

The technically advanced countries with large industrial output not only have an advantage in building a nuclear striking force but also, I believe, are better equipped to withstand and survive a nuclear attack. A technologically advanced country should be in a better position to mitigate the force of an attack by active defense measures. Also, contrary to popular view, the capacity to recuperate should be greater in an advanced, industrialized economy. This advantage has two bases. First, in a highly industrialized economy, the plants in a given industry constitute a longer list of targets. The twenty largest separate steel plants in the United States constitute no more than 50 percent of steel capacity, while in less developed economies five or six plants may constitute a majority of capacity. Second, the greater dispersion of skills and education through the population and the existence of a better developed and higher capacity network of communication and transportation mean that in a highly industrialized economy the reaction to disaster is more likely to be purposeful and efficient than under more primitive circumstances.

The restoration of essential services in Hiroshima was prompt. After the devastating raids on Hamburg, industrial production was swiftly resumed. On the other hand, the mere intrusion of unusual demand for labor and local supplies, because of the presence of foreign military missions during World War II, appears to have contributed substantially to the famine in Bengal in 1943. Thus it seems likely that where the capacity of the transportation network is small, where industrial skills are possessed by only a small fraction of the population, and where modern technology is confined to a minority of economic activities, the economy is more vulnerable to destruction and slower to recuperate from atomic attacks. The opinion attributed to Mao, to the effect that China, with a population approaching 700 million, can withstand an atomic attack because some 350 million would remain if half the population were destroyed, appears to be mistaken as

---

*Under some circumstances, a large advantage in population size can to a degree offset a large disadvantage in technology. Thus Communist China was able in the Korean War partly to compensate for its technological backwardness vis-avis the United Nations by lavish supplies of manpower. However, the Chinese army benefited from access to Soviet technology; so this example is not clear-cut.

well as callous. Equal attacks launched on China and on the Soviet Union,
for example, would have the greater relative effect in reducing Chinese
strength, even disregarding the question of the greater defensive capacity
of the Soviet Union. One hundred hydrogen bombs delivered on target would
be more likely to leave the Chinese population leaderless, prey to disorgan-
ized fighting among refugees for inadequate means of subsistence, victims
of famines and renewed outbreaks of disease, and unable to detect areas of
dangerous fallout. It is true that China might emerge relatively stronger
after a general nuclear war, but only because in a war between the commu-
nist and anti-communist nations, the highest priority targets would be in
the Soviet Union, and not because of China's large population and more back-
ward economy.

Implications of Population for Military Potential Among
Industrialized Countries

Among countries with approximately equal access to modern techno-
logy, the number of men who can be put into military service clearly helps
determine relative potential strength, at least in non-nuclear warfare. The
upper limit of men under arms is established by the population in the age
groups qualified for military service, say 16 to 44. German's initial advan-
tage over France in both World Wars derived partly from greater manpower,
as did the ulitimate triumph of the Red army over the German armies in the
East in World War II.

Demographic considerations also operate in a less direct fashion, influ-
encing the capacity of a country to mobilize its eligible men to divert eco-
nomic resources to military purposes. This capacity depends on: (a) the
availability of alternative sources of manpower for essential work; (b) the
size of the national output--the greater the output the higher the proportion
that can be used for military purposes; and (c) the strength of competing
claims for economic goods--primarily consumption.

The age-and sex-composition of the population helps determine both
the availability of workers to replace men called into military service and
the strength of demand for private consumption. A favorable composition
of the population is one with a small proportion in the ages of economic de-
pendency--say under 15 and over 70. Maximum military support can be
extracted from a population with the highest proportion of producers and
the lowest proportion of non-producing consumers. An advantageous com-
position occurs at the end of ten to twenty years of fertility. Low fertility
means fewer children and fewer children means fewer civilian rations.
Low fertility also means more women freed of pregnancy and child care,
and hence more readily available for work outside the home.

The United States was in the most favorable age distribution position
in its history during World War II so far as capacity for all-out mobiliza-
tion is concerned. The dependency burden (population/persons 17-69)
reached an all-time minimum, more than ten percent lower than in 1920 or
1960 (Coale, 4). Persons of productive age in the early 1940's (i.e., those
who served in the military forces and who produced military material and
provided for the needs of the whole population) had ten percent fewer persons
dependent on them than would have been the case if the current age distri-
bution of the United States had then prevailed. This low dependency burden
was not a trivial advantage in an all-out war effort.

Implications of Population for Military Potential Among
Industrially Underdeveloped Countries

The relative strength for traditional military operations of industrially underdeveloped countries today depends to a large degree on supplies and assistance from countries with advanced industrial and military technology. Jet aircraft, tanks, and modern small arms are typically imported, either in the form of military assistance or in international trade. Since supplies from such sources are independent of demographic factors and since population size has little to do with such other dominant elements as organization, training, motivation, and the existence of military traditions, there is nothing subtle one can say about population and the military strength of underdeveloped areas, as long as they remain technologically and economically retarded. Big differences in population establish differences in strength (India is on this account stronger than Ceylon); but there is little to be said concerning the effect of age-distribution on military potential. Underdeveloped countries will have very similar age distributions since fertility is the principal determinant of the age structure of a population, and all underdeveloped areas have roughly similar histories of sustained high fertility. The age distribution of underdeveloped countries is uniformly less favorable to military strength than any of the various distributions found in industrialized countries. The highest dependency burdens in the world are all in underdeveloped areas.

In any event, the national security of the United States is not (nor is it likely to be) directly threatened by the military strength of any country that remains underdeveloped. More real and immediate problems are the influence of the Soviet bloc in the underdeveloped world and the possibility that unrest, rivalries, and para-military operations in these countries might lead to war.

POLITICAL INSTABILITY IN UNDERDEVELOPED AREAS

The population of the world is divided approximately evenly among countries adhering to the communist bloc, countries allied to the group led by the United States, and neutralist or uncommitted countries. There are industrialized and non-industrialized countries in all three categories. Adherence to these groups is not associated in any simple way with demographic characteristics. or even with the level of technology, or with per capita income. However, political and social instability appears somewhat more common today among the economically underdeveloped countries, and any attachment they have to an anti-communist or neutralist position often seems precarious.

Much of this instability arises from sources having little to do with population. For example, one conspicuous source of instability is that colonial governments are being replaced by new governments. The fact that independence is nearly universally wanted today does not make the establishment of a government in a newly independent country easy. Many of the new countries are poorly provided with leaders having governmental experience or adequate educational qualifications; most of them lack traditions of self rule; and many suffer from linguistic or cultural differences which hitherto have been submerged in common opposition to foreign domination.

Another prominent cause of instability in these countries (where average annual income is typically $100 or less) is dissatisfaction with the pace of economic progress. Slow economic progress contributes to instability because nowadays people in non-industrialized areas expect rather quickly the material benefits that they can see modern technology brings. When actual increases in average income are nonexistent, or barely visible, the desire for immediate progress is frustrated, and the tenure of any existing political regime may be threatened.

At the same time, it must be recognized that economic progress provides in itself no guarantee of political stability and no assurance of continued neutrality, or of continued adherence to the non-communist bloc. Economic progress occurs in the underdeveloped areas only if traditional modes of production are changed. Inevitably old customs and social arrangements must change also. A society that remains bound by old traditions might remain both impoverished and politically conservative. However, the forces that introduce economic progress also bring into being an awareness of poverty and a belief that it can be ended. Quite likely actual progress will be too slow to fulfill newly awakened aspirations. In other words, there is a greater likelihood of political instability (and that communism will be attractive) as a country begins to move from a pre-industrial to an industrialized state than when it remains stagnant in a pre-industrial phase.

It would be monstrous to oppose economic development--to favor continued abject poverty--because of the possible spread of communism. In fact, the urge toward modernization and economic development is becoming so nearly universal that any such policy would be absurd as well as inhumane. The point is that assistance to development given by the United States or anyone else will not necessarily prevent a rise of communist sentiment. Such a rise may be a more or less natural development at some phase of the whole complex change that accompanies industrialization. An offer to help with the stated intention merely of winning people away from communism is often offensive and can be self-defeating. For these reasons it may be more realistic to extend aid to developing countries simply to improve the material conditions of their citizens rather than with the expectation that aid will inevitably serve as an effective weapon against communism.

It is easy to minimize in our thinking the appeal that the Soviet example has to countries hoping to industrialize rapidly. A little more than 40 years ago, Russia's economic circumstances were in some ways very similar to those of the underdeveloped areas today: Russia's population was mostly agrarian, impoverished, and illiterate; her industrial sector, while not negligible, was far from a position among the world's leaders. In spite of an intervening history of civil war, of famines during the period of collectivization, of the bloody purge trials, and of disastrous occupation by the German army, the Soviet Union stands today as the second strongest industrial power and as a serious rival to the United States in technology and military strength.

Our own example often appears less relevant to the low-income countries anxious for rapid development. Although the United States remains the unquestioned leader in per capita income, we appear, especially from the perspective of the underdeveloped countries, to have been wealthy for a long time, rather than having risen recently from an underdeveloped position. Our advantages lie (a) in political freedom, (b) in the increasing

recognition among the nationalistic underdeveloped countries that adherence
to the communist bloc usually means effective loss of independence, and (c)
in whatever economic gains can be achieved by non-communist underdevel-
oped countries.

POPULATION AND INSTABILITY

Population provides some of the reasons why underdeveloped areas
find it difficult to raise levels of living rapidly. The most important ad-
verse demographic characteristic common to pre-industrial societies is
high fertility. High fertility--an average of six or more children born to
each married woman surviving to menopause--was the rule in Europe and
America before industrialization, and is the rule today in the low-income,
agrarian countries of Asia, Africa, and Latin America.

Until recently, high mortality rates--with an average duration of life
of no more than 30 to 35 years--were also typical of preindustrial socie-
ties. The means of reducing death rates and prolonging life--the techno-
logy of modern medicine and public health--evolved in the same countries
where modern industrial technology originated. Thus major reductions in
death rates were achieved first in Western Europe and North America.
The process of reduction was gradual. It as followed, with varying time
lags, by the drastic reduction in birth ra es that all highly industrialized
nations have experienced.

Until about 1920, marked reductions in mortality--the achievement of
an expectation of life in excess of 35 years--appear to have occurred only
where the economy underwent a major process of modernization.

In the underdeveloped areas today it is proving feasible to import low-
cost, extremely effective techniques of modern medicine and public health
from the technically more advanced countries, whether or not the economy
is modernized. As a result, even the most impoverished countries are able,
with modest expenditures, to reduce death rates at a very rapid pace. Re-
sidual insecticides and antibiotics are notable examples of the technical
discoveries that make this progress possible.

With mortality thus reduced, the high birth rates of low-income coun-
tries produce rates of natural increase without precedent in human history.
Singapore, Malaya, and Taiwan; Mexico; and some of the South Pacific is-
lands are growing at annual rates of three percent or more. India and Pak-
istan have growth rates of more than two percent. Doubtless, so do China
and other Eastern countries, the Near East, North Africa, and Latin America.

Accelerated population growth requires correspondingly faster economic
growth to achieve a given rate of increase in per capita output or income.
Imagine two populations identical at a given moment in size, in age distri-
bution, and in production. Suppose one to be stationary and the other to be
growing in population at three percent per year. Naturally the growing
population must expand its output by three percent to avoid a deterioration
in per capita income. If production is to rise by three percent, somthing
like six to twelve percent of current production must be devoted to invest-
ment--to net additions to productive plant and equipment. Therefore the
growing population must achieve a large volume of investment merely to

so even. This same investment would permit the stationary population
to increase per capita income by two or three percent. Thus the rapid
population growth now associated with continued high fertility makes it dif-
ficult to raise levels of living rapidly.

Another adverse effect of high fertility on the growth of income per
capita is the high burden of dependency it causes. Children under 15 amount
to 40 percent or more of the population in the typical underdeveloped coun-
try, while only some 25 percent in the industrialized countries. The dif-
ference in dependency burdens is almost wholly the result of contrasting
fertility histories. In fact, the more favorable mortality experience of the
industrialized countries has raised the proportion of children slightly (ac-
tually offsetting slightly the effects of lowered fertility) because the great-
est differences in survivorship are in infancy and childhood. The high bur-
den of dependency makes it harder to set aside part of current production
for investment in growth in output. High fertility, by causing rapid growth,
increases the volume of investment required to achieve a given rate of in-
crease in per capita income and, by its effect on the age distribution, in-
creases the difficulty of achieving a given volume of investment.

These disadvantages may appear slight compared to more immediate
obstacles that are conspicuous to the short-range planner, However, their
effect cumulates to major proportions in a generation. A steady reduction
of fertility, leveling off at 50 percent of its former value in 25 years, would
in a generation produce an income per consumer 35-45 percent higher than
unchanged fertility. After 50 years, a continuation of reduced fertility would
yield per capita incomes nearly twice as great as those associated with con-
tinued high fertility (see Coale and Hoover, 5, pages 326-329).

The calculations underlying these statements make no allowance for
the effects of greater pressure on resources, or, on the other hand, of
economies of scale. In other words, no allowance is made for "overpopu-
lation." The advantages of reduced fertility are therefore understated for
such large densely populated countries as China and India.

To return to the theme of this section (population and instability in the
underdeveloped areas): High fertility does make it more difficult to attain
a rapid rise in individual well-being. Other obstacles--the scarcity of
leadership in government, education, and industrial enterprise; traditions
of investing in trade rather than in production; inadequate transportation,
public utilities, housing, and "social overhead" in general; problems of
"balance" in development, etc.--are so difficult that rapid increases in per
capita income would often be problematic even if the obstacles created by
high fertility were absent. Conversely, a large gain in agricultural pro-
ductivity can sometimes be realized from such inexpensive expedients as
superior seeds, more careful weeding, and the use, of a little inorganic
fertilizer. Contemporary techniques of manufacture can be adapted to
particular circumstances in the underdeveloped country and can provide
large increases in productivity over traditional handicraft techniques.
These possibilities mean that important progress can often be made in rais-
ing levels of living while the birth rate remains as high as ever. Mexico,
for example, has raised per capita income by more than 50 percent in the
past 20 years while the population has increased by more than 75 percent
and while its high birth rate has not wavered. Nevertheless, sustained
high fertility, by withholding major additional increases in per capita

income that a lower birth rate would bring, persistently contributes to the
disappointment of aspirations.

Another aspect of population that contributes to political instability is
the urbanization that is taking place in many underdeveloped areas. A sub-
stantial increase in the proportion of the population that is urban charac-
teristically accompanies the process of industrialization. Increased urban-
ization is a natural consequence of the nature of modern industry. Cities
usually become the loci of manufacturing. The urban population grows
partly because growing industries provide expanding employment opportun-
ities. At the same time, improvements in agricultural technology make
possible an expanding agricultural output with a stationary agricultural
labor force; and the annual increments to the rural population migrate to
the cities to man the growing industrial sector.

This urbanization-cum-industrialization appears to be occurring, as
might be expected, in those low-income countries that are industrializing
rapidly. On the other hand, urban populations are also an expanding frac-
tion of the total where industrialization is not rapid. Cities are growing
even where manufacturing is relatively stagnant. Nathen Keyfitz (9) points
out that the combined population of Djakarta and Surabaya grew from 875,
000 to more than 5,000,000 between 1930 and 1955, and "...they have at no
time come to include any important amount of manufacturing or other pro-
duction." The cities remain centers of trade and government. As such,
they are not producing a rising volume of goods that can be traded in the
rural sector for agricultural produce.

The cities in the underdeveloped areas where industrialization is
lagging are growing, in many instances, because recent declines in mor-
tality have led to a rapidly swelling rural population while agricultural land
remains limited, not because the cities are providing jobs that attract work-
ers from the rural sector. In other words, the cities are growing because
of the "push" of growing numbers and limited opportunities in the villages
rather than because of the "pull" of expanding opportunities in the cities.

Cities in countries experiencing the early phases of an "industrial rev-
olution" have rarely been noted for ideal living conditions. Nevertheless, as
the industrialized countries have gained in wealth, urban living conditions
have gradually improved. The urban millions in some of the countries of
Southeast Asia, on the other hand, have been uprooted from their tradition-
al place of residence and live in a sordid environment of social disorgan-
ization without the redeeming advantages of higher income or fuller em-
ployment. These urban dwellers are not employed in modern industrial
plants, but continue to work in family-centered enterprises of a handicraft
or market-stall variety rather than in agriculture. With continued rapid
population growth, the proportion of the population living under these cir-
cumstances will tend to increase rapidly. A swelling urban population
living on the street or in houses that were grossly inadequate for much
smaller numbers, hopelessly competing for unproductive and illpaid emp-
loyment, is not a promising environment for political and social stability.

The Communist Position on Population Problems

There is a rather dogmatic communist position to the effect that in a
socialist state (i.e., what we would call a communist state) there can be

neither "overpopulation" nor excessively rapid population growth. According to this view, overpopulation exists when there are more workers than jobs; this imbalance is possible (indeed inevitable) in a capitalist economy only. This position was first set forth by Marx and Engels in their critiques of Malthus, and it remains in essence the official communist position today (see Mauldin, 10).

This Marxist doctrine is applied to the underdeveloped countries. Communists assert that the underdeveloped countries can provide for an expanding population by establishing a socialist society and by planned economic development. Any concern about the effects of high fertility is labeled neo-Malthusian, bourgeois, imperialist, and even cannibalistic. If the opinion is expressed in one of the Western countries that high birth rates in Asia or Latin America are deleterious, the communist response is that Western imperialists want fewer Asians or fewer Latin Americans because of the threat that larger numbers would pose for the West.

Despite this dogmatic position, the actual policies followed in many of the countries within the communist bloc have been associated with rapid and substantial declines in fertility in spite of some pro-natalist moves. In Russia, abortion under state auspices was made legal in 1920 because of the hazards to health inherent to illegal abortions. After a precipitous fall in the birth rate in the late 1920's and early 1930's, abortion was severely restricted in a decree passed in 1936. In the same decree there were provisions for family allowances, maternity benefits, and the like, that appear to have been clearly pro-natalist. Additional allowances (and medals for motherhood) were introduced in 1944. After World War II, the program was continued, although financial allowances for children were reduced 50 percent in 1947. In 1955 the prohibition of abortion was repealed, and the explanatory text of the decree in effect repeated the reasoning of 1920--that clandestine abortions are dangerous to health. Contraceptives are easily available in the Soviet Union in state-operated stores.

During the interval when abortion was prohibited and pro-natalist laws were in effect the birth rate in Russia fell from 38 per thousand (in 1938) to about 25 per thousand (in 1955). The decline of fertility in Russia was very similar to the decreases experienced by other countries that have undergone industrialization. Moreover, the Marxist "line" on population has not caused the Soviet government to interfere with restriction of fertility more than many Western governments have. In fact, the legalization of abortions and the sale of contraceptives in state-operated stores represent a more positive attitude toward family limitation than most Western governments would be prepared to take.

The history of population policy in Communist China is more confusing. Until 1954, any statements from Chinese Communist sources about population followed the orthodox Marxist line. In that year, and the succeeding three years, many statements were issued about the need for birth control, and in the same interval (1954-56) such birth control activities as the sale of contraceptive appliances in government-operated stores and birth control education under party auspices were started. The advocacy of birth control was usually coupled with violently worded attacks on Malthusianism, neo-Malthusianism and the like, and was couched in terms of spacing births to protect the health of the mother, slowing down births until China's facilities for child care were expanded, and so on.

From 1957 to the present, however, the advocacy of birth control has
been under frequent attack by Chinese Communist spokesmen, and o the
surface there appears to have been a return to a more strictly Marxian
position. However, some writers continue to refer to the desirability of
planned population growth, and birth control is still permitted, though no
longer publicly promoted by the party.

Whatever the status of overt population policy in China, the changes
that the Communist government has imposed on the Chinese represent
about as direct an assault as could be devised on the forces that would
tend to maintain the traditionally high Chinese birth rate. When demog-
raphers speculate about why birth rates remain high in underdeveloped
areas, they cite the importance of tradition and custom and the family and
kinship systems as agencies for preserving and transmitting traditions and
customs. The establishment of the communes, with the abolition or severe
diminution of the role of the family as the locus of production and even as
the center for child raising, will (if the communes prove viable) weaken
the influence of the family very directly and rapidly. I would not want to
predict on this account an immediate rapid decline in the Chinese birth rate;
but a decline would be much less surprising than in another country where
traditional family arrangements have been left intact.

The standard Marxist position on population growth is still reiterated
in any policy statement about underdeveloped countries. It is asserted that
population growth cannot be a problem to a socialist society, and that if
planned economic development in a socialist pattern is instituted, popula-
tion will take care of itself. At the same time, some of the policies followed
in communist countries are in fact favorable to the limitation of birth. The
policies having restrictive effects range from legalized abortion in the Soviet
Union to forced social change in China.

Population Policy in the Underdeveloped Countries

The governments of underdeveloped countries are becoming increasingly
conscious of the importance to social and economic development of the size,
rate of growth, and characteristics of their populations. For example, some
countries conducted their first censuses in 1959-61 because of recognized
need for population data in formulating plans and policy for education, hous-
ing, industrial development, and the like.

A few governments have also recognized the handicap that continued
high fertility imposes on their development and have started some sort of
action, or at least are studying possible action, with the aim of bringing
about an early decline in the birth rate.

An effective anti-natalist policy for an underdeveloped country is not
easily formulated. The naive view of an adequate program is merely to
make contraceptive information and appliances available in subsidized birth
control clinics. A variant of this program includes the development of more
effective and acceptable contraceptives (e.g., a birth control pill). Clinics
and more effective contraceptives are indeed important, but experience is
showing that to reduce the birth rate it is necessary to change values: to
educate men and women to entertain the idea of exercising choice in the
number and spacing of their children and to convince them that this choice
is worth some care and trouble. These problems of "communication and

motivation" are intrinsically more intractable than the invention and dissemination of inexpensive, non-objectionable, and effective contraceptives.*

American Policy on Population in Underdeveloped Areas

The proper policy of the United States Government toward the population problems of underdeveloped areas involves some delicate issues. The United States is committed to helping the underdeveloped areas to raise levels of living; and it is widely recognized within our Government that high fertility is in many countries a barrier to the rapid growth of per capita income (see, e.g., the "Draper Report," 7). Nevertheless, the Government must be cautious in favoring lower fertility, or in taking an active part in fertility control programs in underdeveloped countries, if only because sponsorship of fertility restriction might be self-defeating. Communist propaganda would promptly label it as imperialistic, and when the United States tells Indians, Indonesians, or Egyptians, "You should have fewer children," a negative reaction is quite natural. To have the best chance of success, restrictive population policies should be domestic in origin.

In the long run, the greatest contribution the United States can make to assist low-income countries in coping with problems associated with high fertility is to add to the store of fundamental knowledge about population and help transmit this knowledge to the low-income countries. This would require an expansion of basic demographic research in the United States and expanded training of population experts, especially from the underdeveloped countries.

Economic-demographic research has shown the implications of population trends in the underdeveloped countries, including estimates of the gains that would result from reduced fertility (Coale and Hoover, 5). A study not yet published (Demeny, 6) shows how to calculate the maximum expenditure on a family limitation program that will still yield a greater increase in per capita income than alternative uses of the funds.

Rather abstract and general mathematical research on population structures--the theory of "stable" populations associated primarily with the name of Alfred J. Lotka--serves as the basis for our understanding of how the rate of growth and the age distribution of a population are determined by the course of mortality and fertility. Recent elaborations of this theory have made it possible to estimate, from fragmentary census date (see, e.g., Bourgeois-Pichat, 2), for many underdeveloped areas, such important population measures as the birth rate, the expectation of life at birth, and the detailed age distribution.

Additional research in the United States on communication, motivation, and on the specific factors affecting fertility can ultimately be useful in

_____

*President Ayub of Pakistan. in a news conference at the National Press Club on July 13, 1961, in Washington informally proposed a simple short cut to the problem of motivation. "You should have a pill or something and make people eat it and that would be the end of it," he sa   (New York Times. July 14, 1961). However, forcible control over something as personal as parenthood would in most countries be objectionable in princip , and would no doubt arouse violent opposition.

predicting population changes and in providing underdeveloped countries with
the means for formulating population policy. For example, a careful hist-
orical analysis of declining fertility in the long list of diverse countries
and regions where the birth rate has dropped might provide important clues
about the relative importance of education, urbanization, occupation, and the
like, in causing reduced fertility. Another possibility of useful research is
experimentation with techniques of education and persuasion among segments
of the United States population that do not plan the size of their families (see
Brøgue, 1). Anything learned from these high fertility groups about motiv-
ation and communication with respect to birth control may prove valuable
in the underdeveloped countries.

Most of the research on motivation and communication should, how-
ever, be conducted within the underdeveloped areas themselves. This re-
search requires experts trained in survey techniques, statistical methods,
psychology, and sociology as well as demography. Moreover, these coun-
tries need persons trained in demography for their censuses; their regis-
tries of births, deaths, and marriages; and in such ministries as health
and labor, as well as for research. The training job, therefore, is a major
one. International training programs have already been started. For ex-
ample, The Population Council of New York has a program of special fel-
lowships that permits a modest number of students of population from Asia,
Africa, and Latin America to study population in the United States or Europe.
Also, demographic training centers are being started under United Nations
auspices. One has been established in Santiago, Chile (for Latin America),
and one in Bombay (for Southeast Asia). Probably another will be started
in Africa within a few years. But the flow of trained students from these
programs is far short of the needs.

An expansion of basic demographic research in the United States is
amply justified on the humanitarian grounds that it would increase the hope
of dealing with one of mankind's most serious prospective difficulties. Ex-
pansion can also be supported on grounds of national security because unre-
solved population problems add to the political instability of underdeveloped
countries, and because of the ideological implication of failure in the
economic development of various of these countries.

It should be re-emphasized that the reduction of fertility is not the only
population problem in the underdeveloped countries. Even if the birth rate
were to start to fall immediately, these countries would still face many
years of rapidly increasing numbers in the labor force, which is already
plagued with unemployment and underemployment. Projecting manpower
supply and needs, analyzing rural-urban and other forms of internal migra-
tion, and estimating school enrolments are a few of the topics on which
demographic research and analysis are needed. These problems are also
intimately related to the stability of the underdeveloped areas. I have em-
phasized the problems associated with the continuation of high fertility be-
cause in the long run they are the most difficult problems.

DOMESTIC POPULATION PROBLEMS AND POLICIES

The whole quality of the future development of the United States will be
affected by the development of its population. This development, in turn, is
determined in part by characteristics of the population that have been

"built-in" by the past sequences of events and in part by future trends in
fertility, mortality, and migration. For example, the present age distri-
bution, ethnic composition, and locational pattern provide the basis for
future change, and are themselves the product of past trends of births,
deaths, and migration (both internal and international). Some of the con-
spicuous problems arising from the current demographic situation and
trends are discussed below.

Long-range overcrowding. The remarkable upward turn in the birth
rate since the 1930's, combined with continued progress in controlling
various causes of death, has produced a rapid rate of increase in the popu-
lation of the United States. A continuation of present fertility and mortal-
ity would cause the population to double about every 35 years.

If continued for a very long time, the current trends in family size
would be disastrous. They would give the United States a population of a
billion in less than a century, and a population greater than that now
occupying the whole world in about 150 years. It is unlikely that fertility
would remain at high levels once overcrowding has become a conspicuous
source of discomfort in individual households. However, it seems an unnec-
essary hardship for the nation to let population growth proceed unchecked
until individual couples feel that they cannot afford to raise more than one
or two children. It would be irrational for an individual couple to restrict
family size because the national population was large--as if an individual
voluntarily gave the government more than his proper income tax payment
because of threatened inflation. Long before the population became so
large that individual couples would feel compelled to reduce their planned
family size from three to two, for example, overcrowding would have
become a major disadvantage to everyone because of traffic congestion,
swarming recreational facilities, and steeply rising costs for every
function requiring space.

Thus we cannot necessarily count on rational individual self interest in
the determination of family size to keep the birth rate at a nationally advan-
tageous level. The reduction of fertility in the United States is not a matter
of introducing deliberate birth control. Ninety percent of white American
couples with no sterility or fertility impairment use some form of deliberate
control of fertility when the wife is over 30 (see Freedman et al., 8, page
66). There are still couples who, because of ignorance or inadequate moti-
vation, have no effective conscious choice in the number of children born,
but these couples are a small and diminishing minority. Our high birth
rate today is sustained primarily by couples who know about contraception
and are fairly proficient at achieving the number of children they delib-
erately choose. The number they want (mostly 2, 3, or 4) suffices to
keep the rate of population growth high.

While overcrowding of a very painful or overwhelming nature is not an
immediate prospect, it is not too early for consideration of possible national
policies that would retard growth before it becomes seriously harmful.

A continued rapid growth in school enrolments. Primary school en-
rolments rose by 11 million in the 1950's and will rise by another 5.5
to 6 million during the 1960's. High school enrolment, which rose by
4.6 million in the 1950's will increase by an additional 3.9 to 4.6 mil-
lion; college enrolment, which increased by 1.4 million in the same

period, will increase by another 1.7 to 3.4 million.* If proportions attend-
ing college continue to expand, total college enrolment in 1970 would be
9.3 percent higher than in 1960.

In the early 1950's the primary schools were forced to deal with the
large post-war birth cohorts, and by the late 1950's these cohorts were mov-
ing into high school. High school expansion will continue in the early 1960's ,
and by 1970 the colleges will have felt the full impact of the postwar baby
boom. The difficulties of maintaining standards of teacher qualifications,
of students per class, and of educational plants will be transferred to high
schools and colleges during the 1960's. It will be hard to avoid deteriora-
tion in the average quality of secondary and university education at the
very time that many feel a substantial improvement in quality is needed
for national defense as well as for national economic and social development.

Increasing problems of congestion and inadequate organization in the
major urban areas. In 1960, the population in metropolitan areas of more
than 100,000 persons was 62 percent of the national population. The number
inhabiting areas of this sort was 76 percent greater than in 1950. Another
indication of the increasing concentration of population is the fact that more
than 50 percent of the counties in the United States lost population in the
1950's. The metropolitan area increase was especially marked in outer
rings. The centers of the larger cities grew very little and in many in-
stances lost population. This "urban sprawl" will continue to multiply
the number of governments that deal with each metropolitan area's prob-
lems, will add to traffic congestion and time spent in the journey to
work, and will contribute to the disappearance of accessible open coun-
try as the boundaries of the great metropolitan areas continue to ap-
proach each other.

A rapid increase in the number of new entrants to the labor force. In
the 1950's young people reaching age 17 were born in 1933-43, when births
were at, or near, a low point. In the 1960's the young people born during
the postwar baby boom will be entering the market for jobs. At the time
when automation is being introduced on a wide scale, the economy must
provide a record number of new jobs for young people starting their careers.

A continued increase in the aged population. In spite of the recent high
fertility, which has swollen the proportion of children in our population, the
proportion of the aged has continued to increase. The proportion of persons
over 65 has increase from 4.1 percent in 1900 to 8.8 percent in 1960 and is
expected to increase still further to about 9.2 percent of the population by
1970. Moreover, because mortality rates in the older ages have recently
improved much more rapidly among women than among men, the older pop-
ulation is becoming increasingly female in composition. It is not easy to
conceive an adequate role for the aged in an urban society where separation
of adults from their parents at marriage is a custom approaching univer-
sality. The current controversy over medical aid for the aged exemplifies
the sort of problem that will be increasingly pressing in the 1960's.

---

*In each instance, the smaller estimated increase for the 1960's assumes
that enrolment rates will remain at 1960 levels, while the larger increase
assumes a continuation of increasing proportions enrolled.

It was suggested earlier that increased graduate training and basic research represent the best contribution that the United States might make in helping the underdeveloped areas to cope with their population trends and the implications of those trends. The domestic problems just described provide another reason why training and basic research in demography should be accelerated.

DEMOGRAPHIC RESEARCH AND PROBLEMS
OF NATIONAL DEFENSE

The reader interested in military problems may feel that the national and international questions to which so much of this essay is devoted are, in a professional sense, none of his business. They have nevertheless been included both because of their ultimate relevance to national security and because of the high priority that should be given to increasing the number of trained demographers and to expanding basic population research.

There are also national defense problems whose solutions depend more directly on population research. I shall describe two instances briefly: (a) the formulation of selective service plans and (b) estimates of the present and future economic and military strength of the Soviet Union.

Selective Service Plans

An elementary consideration in selective service plans is the number of young men of draft age, classified by educational and marital status. For 18 years in the future the number of persons at individual ages between 18 and 25 or 30 can be estimated quite accurately because they well be survivors of persons now alive and will be passing through a part of life in which mortality rates are very low. At current mortality rates 93 to 94 percent of males survive from birth to age 30. Even fairly large reductions in mortality rates could not cause consequential errors in the estimates. The estimation of proportions attending college and being married is more difficult. We know that there has been a steady upward time-trend in college attendance rates, but in the next decade this trend may be restrained by the difficulty of expanding educational plants and faculties. The Bureau of the Census (3) has published a set of projections of the attendance at institutions of higher learning until 1960, on various assumptions.

The proportion married at ages 18 to 25 changed radically during the decade 1940-50 because of a decline in the average age at marriage and a major increase in the proportion ever marrying. No similar strong trend was observed in the 1950's, but the possibility of a drift back to the marriage habits of earlier generations remains a source of uncertainty.

Strength of the Soviet Union

Population helps determine the present and future strength of the Soviet Union. With information derived from the published results of the 1959 census, it is possible to forecast accurately until 1974 the population over age 15, subdivided into 5-year age-groups for each sex. From this basic projection the number of males and females available for participation in the labor force, and for military service, can be inferred. The total population of ages 15 to 64 will increase by an average of less than 1 percent

per year in 1959-64, and by nearly 1.6 percent per year in each of the sub-
sequent two 5-year periods. However, the male population of ages 15 to
64 will increase more rapidly (1.4 percent, 2.2 percent, and 2.0 percent
per year in 1959-64, 1964-69, and 1969-74) because the older male groups
passing the age of retirement are very small, having been depleted by two
World Wars. In fact, the population in the labor force ages will gain in
masculinity from a ratio of 75.8 males per 100 females to a ratio of 89.5
between 1959 and 1964.

The number of young men of military age (say 15-29) will actually de-
crease by 16 percent during the period 1959-64, and will still be slightly
below the 1959 level by 1969. This decline is caused by the very sharp fall
in births and the high infant mortality that occurred during 1940-45. The
"trough" in the Soviet age distribution caused by these deficient cohorts is
now moving into the ages of military service. The trough will be centered
on age 20 in 1963-65.

Thus there is at present a period of slow growth in the Soviet popu-
lation of labor-force age, a period that will be followed by ten additional
years, at least, of moderate growth. Moreover, the groups usually employed
in military service--the younger labor force age-groups--are contracting
sharply, and will not regain their 1959 size for another decade.

These facts make a difference in the kind of plans the Soviet author-
ities must make. The most conspicuous adjustment they must make is to
the shortage of the persons now 14 to 23 years old. This shortage means
that there are few persons at the normal age of apprenticeship for every
kind of occupation. Moreover, "apprenticeship"--including advanced ed-
ucation and professional training of engineers, scientists, and doctors--
must compete with military service in this age-group. Soviet authorities
have met this shortage by reducing the proportion of young persons in day-
time attendance at high schools and universities and by a reduction in the
armed services after 1955.

Increase in Soviet military forces will be especially difficult because
of this shortage. If a large increase is effected, it will be difficult to
avoid interfering with an adequate flow of trained persons for the needs of
the economy.

Many other inferences about the Soviet economy and society can be
based on demographic data. The fact that nearly 50 percent of the labor
force remains in agriculture (compared to less than 10 percent in the
United States) is one of the symptoms of the failure of the Soviet agricul-
tural program. From a detailed cross tabulation of geographical location,
age, sex, and occupation, it is possible to infer much about the location and
even the approximate capacity of industry. Cross-tabulation may some-
times yield such additional facts as the existence of labor camps, the size
and age-composition of the armed services, and other information that does
not appear explicitly in the published tables.

These possibilities may explain the reluctance of the Soviet Union
to publish detailed tabulations from the 1959 census and of China to
publish details from the census of 1953.

THE NEED FOR SUPPORT IN DEMOGRAPHIC
RESEARCH AND TRAINING

There are two basic factors inhibiting growth in the number of social scientists with a firm grounding in population. One is the limited number of institutions where a strong program of demography is available in the postgraduate curriculum, and the other is the rather small number who elect to study demography at the universities where it is offered.

At the moment, intensive graduate training in population is available at only a dozen of the first-rank American universities. At these institutions, a graduate student in sociology or economics may be exposed more or less fortuitously to demography, and, finding it congenial, make it the focus of his interest. But at Harvard, Massachusetts Institute of Technology, Yale, John Hopkins, or Stanford, to name some specific examples, this choice could occur at best very rarely because little or nothing of demography is taught. Consequently, students with the intellectual qualities to become leading demographers are never really exposed to the possibility. The number and average quality of demographers ten years from now would be very substantially greater if a majority rather than a small minority of the leading universities had active centers of demographic training.

Even at universities where adequate training is offered, the flow of social science graduates who specialize in population has been rather a thin trickle. The volume will doubtless increase gradually as the shortage of trained persons makes itself obvious in conspicuously good job opportunities and as the significance of population problems continues to command so much public attention. But a more immediate and certain acceleration could be achieved by offering a modest number of generous fellowships to graduate students who make demography one of their fields of study. Many students have decided only on the general discipline (e.g., economics or sociology) they wish to follow at the time they enter postgraduate work. The specific fields they study, and the area in which they concentrate, are determined by somewhat fortuitous events and influences. A powerful factor is financial support. When a generous fellowship is available to students showing an interest in a certain area, it is a safe prediction that many of the best students will apply. Some of them will then be strongly and permanently attracted by the subject.

As stated earlier, given the current scarcity of research personnel, there appears to be no crucial shortage of financial support for demographic research. If viable demographic programs are to be established at universities now without them, as I believe is imperative for the future of the field, there must be much more academic research than there is today. It may develop that the sources of financial support available at present will be insufficient and that the national interest will require expanded government support for basic research. The need for and value of such support will be greater when the supply of talented demographers has been enlarged.

To recapitulate the most essential needs as I see them:

a. Demography should be incorporated in the postgraduate curriculum at more universities.

b. A modest number of fellowships should be made available to social science graduate students who are willing to include solid training in demography in their program. Additional financial support for students from underdeveloped areas is also needed.

c. Financial support should be available for research at the universities newly offering courses in population and for the gradually expanding volume of basic research as the number of qualified research workers increases.

Government support of research on a large scale is not a general need until the number of demographers increases as a result of accelerated training. It does seem appropriate for the government to set up a small number of fellowships for graduate students with an interest in demography and to encourage the incorporation of training in any demographic research it sponsors at universities. Within a few years, federal support of basic research in demography will doubtless require expansion. However, for many years, in spite of expansion, the personnel in the field will remain so few that the funds involved will remain a trivial fraction of the total federal expenditures in support of scientific research. But the only avenue for clarification of important issues, for the better comprehension of our prospects, and the wise formulation of population policies is to broaden the scope and improve the quality of basic research in demography.

REFERENCES

1. Bogue, Donald J. "The Family Planning Attitudes of Negro Women in Chicago," Population Index, (1961), pp. 207-208.

2. Bourgeois-Pichat. Jean. "Utilisation de la notion de population stable pour mesurer la mortalité et la fécondité des populations des pays sous-développés," Bulletin de l'Institut International de Statistique (Stockholm), 36, Part 2 (1958), 94-121.

3. Census Bureau. Current Population Reports. (Series P25, No. 22, June 22,1961.) U.S. Bureau of the Census. Washington, D.C.: Government Printing Office, 1961.

4. Coale, Ansley J. "Population Change and Demand, Prices, and the Level of Employment," in Demographic and Economic Change in Developed Countries, National Bureau of Economic Research. (Pp. 352-370). Princeton, N.J.: Princeton University Press, 1960.

5. _____, and Hoover, E.M. Population Growth and Economic Development in Low Income Countries. Princeton, N.J.: Princeton University Press, 1958.

6. Demeny, Paul. "Resource Allocation and Population Growth." Unpublished Ph.D. dissertation, Princeton University,

7. "Draper Report." Third Interim Report Submitted to the President of the United States by The President's Committee To Study the United

REFERENCES (Continued)

States Military Assistance Program, July 13, 1959. Washington D.C.: Government Printing Office, 1959.

8. Freedman, Ronald, et al. Family Planning, Sterility and Population Growth. New York: McGraw Hill, 1959.

9. Keyfitz, Nathan. "The Ecology of Indonesian Cities," American Journal of Sociology, 66 (1961), 348-354.

10. Mauldin, W. Parker. "Population Policies in the Sino-Soviet Bloc," Law and Contemporary Problems, 25 (1960), 440-507.

11. Renouvin, Pierre. "Demographie et relations internationales," Population, 15 (1960), 625-654.

VIII

GAMING AS A MILITARY RESEARCH PROCEDURE

Vincent V. McRae

Gaming as a part of military preparations for war or for the main-taining of military establishments strong enough to deter war is certainly not new; it is probably as old as organized warfare itself (37). However, as gaming activities of the past are examined, one does not see the diver-sity of applications, or the same relative total effort, or the sophistica-tion of approach that is being employed today. Today's military games seek to assist in the engineering of equipment, to facilitate its adaptation to human use, to test and develop tactics and strategy for military units of practically all types and sizes, and even to assist in the formulation of na-tional security policy. But combat problems in the narrow sense are no longer essentially the only application of gaming techniques. Economic systems that support the military establishment are being studied through gaming, as are the political systems and interactions that give purpose to and impose constraints on the use of military force, and still other topics. Even a partial list of the objectives of gaming, today, would have to include the following:

(a) Training of personnel (34, 38, 43);

(b) Examination of the completeness of preliminary solutions to com-plex problems that involve human behavior (particularly that of command decision) and interactions that are not too well under-stood (3, 34, 38, 40, 43);

(c) Gaining of insight into the interactions between variables in com-plex problems involving human behavior or in very complex problems not necessarily involving a significant human parameter (3, 9, 13, 16, 17);

(d) Isolation of principal issues involved in complex problems and the definition of these in terms that will permit later and more de-tailed study (33, 38, 43);

(e) Demonstration to high authorities or the users of research results of the validity of the solution of a complex problem that has been developed by highly sophisticated analytic techniques (18);

Vincent V. McRae, PhD, is an operations analyst with a background of mathematics and physics. He is at the Office of Science and Technology on leave from the Research Analysis Corporation. During World War II he served as operations officer of an Army counterintelligence unit. Prior to his present assignment his work as operations analyst has covered the top-ics of tank warfare, tactics, air defense, military strategy and balance of armed forces, arms control, civil defense, and war gaming. In 1958 he was a member of the U.S. delegation to the Surprise Attack Conference in Gen-eve and in 1959 he served as research associate for the Coolidge Committee.

(f) Collection and analysis of relevant data on many numerical aspects of combat, particularly logistic, but including casualty, spatial, terrain, and time aspects (3, 25, 43, and Adams and Girard in 33);

(g) Exploration of the effects on plans, tactics, doctrine, etc., of the possible reactions of a skilled and determined enemy (17, 30, 43);

(h) Stimulation of innovations and inventions of new tactics, doctrine, weapons, weapon systems, policy, etc. (17, 27, 33);

(i) Determination of boundary conditions for lower-level games (e.g., political conditions for segments of theater-level games) and of assessment models (or inputs) for higher-level games;

(j) Provision of an effective setting for the collaboration of the specialized individuals involved in politico-military analysis (17, 22, 24, 27, 43),

(k) Provision of a systematic means of adjudicating conflicting claims and lines of argument (27, 43);

(l) Gaining of insight into the effects of pressures, political uncertainties, moral and cultural factors, and intellectual difficulties on proposed courses of action (23, 43);

(m) Synthesis of solutions to parts of a complex problem (38).

There are, of course, other objectives that may be achieved by individual games. Those listed appear not only to be frequent but also to receive wide degrees of acceptance.

Many attributes of gaming contribute to the achievement of these objectives, and from the functional description of gaming which follows below, it will become obvious that there is great room for differing degrees of achievement owing to the many subjective factors that enter into the performance of the functions discussed. But the very act of structuring a situation in the detail necessary for gaming simulation is a form of analysis as well as of synthesis, for it forces consideration of interactions and examination of the significance of numerous variables and the effects of human decisions, all in a dynamic context that is much closer to actual occurrence than is the realism achievable in most of the more formal types of analysis. This is not to belie the utility of more formal analysis, for in fact many of the known techniques used in such analyses are exploited in the various models, which are often much more than mere sets of rules and may be rigorously defined and evaluated mathematically or may be physical models of components of the problem under study; this is equally true of various types of postgame analysis.

The remaining aspects of gaming that appear to contribute importantly to the achievement of the types of objectives listed above seem to be the manner in which individual and collective judgments are brought to bear on the problems under study and the way in which imagination and innovation are stimulated. These play important roles in objectives (b) to (e), and (l), and are almost directly responsible for the ability to obtain objectives (g)

and (h). The excellence of judgment brought to bear is directly related to the experience and ability of the players obtained, but the stimulus to innovation comes from the nearness to reality that is obtained and from the competitiveness inherent in the gaming situation. Both these factors vary widely with individual games, as do the objectives that actually can be obtained to any specific degree.

Whereas at an earlier time games were formulated and used by a few military specialists with comparatively little effort or cost, today's games require more military personnel, utilize the skills of numerous types of scientists, incorporate the results of the recent advances in computer techniques and in electronic and communication technology, and consume sizable fractions of research and development funds.* Several major operations research establishments** devote approximately 5-25 percent of their resources to activities that are properly classified as gaming, computer simulations included. It is the diversity, the sophistication, the large fraction of the research and training effort of the United States military establishments that are currently being devoted to gaming that give importance to this study.

Gaming activities are carried out by teams of scientists with significantly different backgrounds and from widely different disciplines. Specialists from most areas of science, including the technical sciences, are engaged as consultants with increasing frequency. Having participated in and supervised some of these activities, I have developed the view that social skills in gaming can be much more fully utilized and I shall endeavor to support this view herein. However, not being a social scientist per se, it is only with some reluctance that I have undertaken to include in this study consideration of the role of social science in gaming, even as a secondary objective, and I present this consideration with some diffidence.

One cannot really understand the need for social science skills in gaming by reading summary gaming reports or by taking uncritically the views of the typical user of gaming reports. Nor can one understand gaming and develop an appreciation of its utility as a research technique in this way, for gaming reports are generally quite technical, brief, and inclined to omit many of the significant details necessary for such understanding.***

---

*See "War Gaming for Signal Systems" (5) for a training game whose development is estimated to require 5 years and to involve substantial funds.

**Such as RAND Corporation, Research Analysis Corporation, Weapons Systems Evaluation Group, and Operations Evaluation Group.

***In forming many of the judgments presented herein I have been aware of the methodological sections of gaming reports that are highly classified because they deal with sensitive military problems. Although methodological studies are not normally classified, their inclusion in classified reports (as is frequently the case in the research of the various military establishments) makes it impossible to provide some of the readily available documentation for many of the statements made hereafter.

Some users and potential users of gaming reports appear to take extreme positions: either gaming is the solution of all problems, and any problem that has not been supported by gaming has not been adequately researched, or, since gaming is involved, the study results are automatically rejected. Obviously many users do not have these extreme reactions and are in fact able to evaluate the results obtained with precision and clarity; yet the extreme reactions are met with sufficient frequency to warrant caution and critical review.

Surely there is some utility to a technique that has gained such wide acceptance in the operations research professions and is receiving active consideration as a research technique in the area of political science (7, 42). However, there must be some real limitations to that utility since within many professions there are well-qualified persons who point out limitations and readily accept more conventional techniques of analysis when available (6, 8, 15, 30, 33, 35). This study supports the judgment so expressed and attempts to clarify some of the limitations as well as some of the areas in which gaming has clear and consistent utility as a research procedure.

It is not intended that gaming be reviewed in complete detail herein, nor that its utility as a research technique, a somewhat more recent usage, be analyzed with great rigor, because this study was not prepared specifically for the gaming expert, nor for the technical evaluators of their work. Rather, it is intended for persons seeking to understand what gaming is and thus to develop an appreciation of its uses and limitations as a research technique. It is also hoped that such persons will develop a better understanding of the role of the social sciences in the rapidly increasing attempts to use gaming techniques in the study and analysis of problems of great scope.

It would be well if these objectives could be obtained without going into the details of how a game is prepared and how it is run. Unfortunately this appears not to be possible, for the development of an appreciation of the utility of gaming as a research procedure requires a fairly critical appraisal of the factors that determine the validity, reliability, significance, etc., of the results to be expected from games. Since these characteristics of the gaming results are significantly influenced by both game preparation and play, it will be necessary to examine them in some detail.

To do so and to really give adequate treatment to the limitations inherent in the gaming process, considerable attention is devoted to a functional description of gaming. This is necessary in order that those not very familiar with the details of the process can understand some of its intricacies;* but since the degree of comprehensiveness that can be achieved is limited, some aspects of these functions are not treated. Following the functional description of gaming, the usefulness of gaming as an isolated research technique is discussed. It will be noted that the somewhat informal use of gaming in more complex analyses, particularly its general use in the

---

*This is an attempt to substitute partly for the experience in game development and play that Weiner (43) suggests as being valuable in appreciating the utility of gaming techniques in research.

cycle of military R&D, makes its total usefulness difficult to ascertain.
Finally, some remarks are made about the future of gaming and the social
sciences; these reflect my conviction that closer association of the two will
be mutually profitable.

Before treating these topics, however, it is necessary to define several
terms and to indicate the various types of games to which the functional
description applies.

TERMINOLOGY

In order to explore the subject of this study without undesirable am-
biguity, it is necessary to attempt to define several terms that are used
quite frequently. "Gaming" is sometimes left undefined (Pennington, in 33),
although it is generally regarded as a type of simulation involving human
decisions. The term "simulation" is used in its broader context to mean
imitate, represent, etc., although perhaps in idealized or simplified form.
In this broad context a system is simulated if a second system is con-
structed so that it behaves like the first system at some level of detail or
abstraction and with some degree of accuracy. The term "system" is left
undefined, but it should be pointed out that this cannot be reasonably done
for the term "simulation," since simulation is often used in a sense that
makes it a synonym for some types of gaming.*

In the context used by many operations analysts, a simulation (model)
refers to a representation of an operating system in which the only human
decisions made are those defining the inputs of the system (Hendrickson,
in ²3). Most often these inputs are defined at the beginning of the opera-
tion of the representation, although the system represented may permit
inputs to be inserted at several time intervals. Since it is theoretically
possible for a single person to define all inputs, I shall avoid the use of the
term "simulation" in this narrow sense and, using game-theoretic termin-
ology, will refer to this type of system representation as a one-person
game. In deference to popular usage, however, the term "computer simu-
lation" is used in referring to one-person games in which all non-decision
operations are accomplished on a computer.

The more complex and challenging types of gaming involve the repre-
sentation of systems in which all the decisions are not normally made by
a single person, or a single group of people, and in which some degree of
competition is involved; these are referred to as multiperson games and
receive emphasis in the discussions that follow. The most typical example
is the two-person or two-team representation (two competitive teams), and
it is to this representation that the unqualified term "war game" usually
refers. Included in the term "war game" are those multiperson games
representing wars in which several nations are involved and in which the
problems to be addressed involve relations between allies or study areas
in which the interests of allies conflict and two-team resolution is not
practicable.

---

*See Orcutt (32) for a discussion of the various connotations that the
term "simulation" has taken on since 1950.

TYPES OF "MILITARY" GAMES

Games have been developed to treat almost every aspect of the full range of military problems, and they have been classified in many different ways. All classifications are not covered in this brief treatment, but some appreciation of the range of subject matter that they treat is required so that the functional description may be placed in context.

Even though only some aspects of many problems can be properly treated through gaming technique, coverage of the full range of military problems is impressive indeed, for, with the increasing application of a rapidly advancing technology to military problems, the range of military interests has come to embrace essentially all aspects of human knowledge. At the lower end of this range one finds the individual and the equipment that he must operate. The individual—the soldier, sailor, marine, airman— is still the basic element in the military establishment. His behavior and interaction with the equipment he must operate and with other individuals with whom he must fight pose problems in behavioral science and human engineering that will always consume much attention. All aspects of the modern physical sciences and engineering enter into the design of the equipment that he must operate, and the man and the machine together pose problems that are very complex. Gaming is said to have utility in analyzing these problems, and one finds military games at this level of activity.

As one enters the field of combat operations (or training for them, or research and development) both the scope and complexity of operations increase, and economic, sociological, and political factors assume increasing importance. The development of a single piece of military equipment can now determine whether specific local communities will be able to maintain their standards of living or must dissolve and change well-established patterns of local societies. It can also predispose certain political policies through its very existence and through the resulting demands for its deployment in certain foreign areas. Or its existence may become so minatory to the security of other nations that they may change fundamental values and patterns of society in attempts to minimize its effects. The complexity, scope, and breadth of military problems seem to know no bounds, and this is reflected in the types of game being developed and used.

Without attempting to be exhaustive in the matter of how military games are classified, it is readily seen that they differ according to purpose, subject matter, and technique of play. The most common descriptive word used is "war," but games tend to be identified according to those aspects that assume the most importance in the view of the gaming groups. Thus one finds "military" games classified as follows:

(a) Purpose: training, research, demonstration, and exploration;

(b) Subject matter: tactical, strategic, logistic, intelligence, communication, transportation, managerial, political, economic, and cold war;

(c) Technique of play: hand, computer assisted, computer, rigid rule, optimal strategy, free play, fixed strategy, closed information, and open information.

It is clearly impossible in this short study to treat in detail games having characteristics as obviously different as the above descriptions imply; in addition, further descriptions could be listed. Consequently the points that games have in common (rather than their points of difference) are examined. An attempt is made to arrive at some generalizations about gaming as a research technique. Obviously there will be important attributes of particular types of games—objectives achievable by them individually and their limitations—that are not treated here; however, games are sufficiently similar fundamentally to make the proposed approach worthwhile.

FUNCTIONAL DESCRIPTION OF GAMING

If one is to reflect on the utility of gaming as a research technique, one must examine a set of functions that differ in detail in individual games but appear to be common to essentially all games. Since these elements are common to most games, including simulations or one-person games, with only slight modifications of form, they appear to constitute the basic structure of the process of gaming. As such they must be understood, at least in general, and their characteristics must be used as a partial basis for an evaluation of the technique. Furthermore, the general character of functions, the manner of their performance, and the skills required to perform them adequately will provide insight into the role of social science in gaming.

In essentially all games examined one finds the following functions being performed:

(a)   Establishment of purpose;

(b)   Predevelopment analysis;

(c)   Establishment of procedures for game operation and rules of play;

(d)   Establishment of models or rules of assessment for the actions to be taken by the players during actual play;

(e)   Preparation of scenarios for individual plays;

(f)   Selection of players and formation of teams;

(g)   Play of the game;

(h)   Analysis of results obtained.*

It should be noted that some of these functions are often performed at one extreme quite informally or perfunctorily and equally often at the other extreme with thoroughness and great deliberation and that these functions may be performed in series in the order listed or to some extent in parallel.

---

*See Beverly (8), Smith and Marney (38), and Weiner (45) for generalized discussions of gaming methodology.

In the following paragraphs the functions are discussed and some of their characteristics that are relevant to an evaluation of the technique of gaming are pointed out.

Establishment of Purpose

The decisions that dominate the game-preparation cycle relate to the purposes to which the game is to be put. These decisions determine the type of operations to be simulated, the emphasis on various aspects, the degree of aggregation, and in general the effort that the gaming group can afford to place in game development. The effort involved is often underestimated, and the costs of gaming development operations tend to restrict the types of games developed and to determine the purposes to which they can be profitably directed.

Since the development of an appropriate game to solve a single noncontinuing problem seems to require the expenditure of effort and energy on a scale so large as to be rarely justified, the purposes for which games are developed, in sharp contrast to being played, are generally multiple, and each is often broad in scope. The most common use of gaming is related to the training of officers, and this objective is frequently included among the purposes; however, even in the various war colleges and staff schools, where training is most prominent, the purposes extend beyond this and include investigative or research objectives. Much more typical are purposes related to the specific operational examination of problems or aspects of combat at various levels of organization or command.* Thus a game may be often regarded as a general tool, and the job to be done must be carefully specified before the tool can be properly designed; however, once the tool is designed it may be put to use in situations not foreseen or contemplated.

The purposes toward which gaming activity may usefully be directed have been listed above and are considered again in a later section treating limitations; however, some characteristics of the purposes for which games are designed and used deserve special comment. Important among these are: (a) the fact that all games involve the element of human decision and (b) the fact that they are usually associated with problems or problem areas in which nongaming techniques are judged to be relatively unsatisfactory because of context deficiencies.** These facts are not entirely unrelated, since it is the human response that introduces much of the complexity in problems, and this complexity generates the need of context. The types of decision and the need for context vary considerably in purpose leading to one-person and multiperson games.

One-person games. In the case of the one-person game, the need for context is minimal, and all decisions are often made at the time that the

---

*The United States Army War College Analytical War Game is a good example of this type.

**For example, Beverly (8) cites the following reasons for using humans in communications-system simulations: "(1) certain parameters of human behavior were unknown, (2) human behavior was too complex for the computer, and (3) face validity--generalization from laboratory to real-world logistics systems." Such reasons often motivate multiperson gaming and set its purpose.

initial inputs to computational models are determined. However, games
have been developed that require the single player to make almost continu-
ous adjustment of various parameters throughout the play or that require
him to do so at various intervals (Leutert, 28, and Adams, in 33). As one
might suppose, the results of these decisions are reflected in a change of
situation that either forms the basis for a new set of decisions or provides
the output necessary for the postgame analysis. Whether the decisions are
confined to initial inputs, are continuous, or are intermittent, the fact that
they can be made by a single person, theoretically at least, and that the re-
sults of such decisions are processed through a model that presumably has
resulted from full comprehension of all pertinent factors and their inter-
actions--thus removing any additional variability due to human factors--
often lead to greater confidence in the predictive value of the game, and
hence to the very extensive use of such simulations. This confidence is
not always justified, as shall be noted, for if such games are examined
closely it is often seen that the human factor has been suppressed, but not
eliminated.

On the one hand games like the air-battle simulation model developed
at the RAND Corporation and extended and exploited by the Air Force
(Daniels, in 33) seek to minimize the effect of human participation and to
concentrate on the characteristics of weapons and weapon systems. This
model simulates in great detail an intercontinental exchange between the
United States and the Soviet Union. It requires an extremely large number
of inputs that must in many instances be estimated by experienced officers.
Consequently, although the stated purpose of a play may be to investigate
weapons and weapon systems, it is difficult, if not impossible, to eliminate
the influence of persons who make such judgments. This is not to say that
such elimination is undesirable or that such models cannot achieve very
important objectives; they obviously can do this. But it is necessary to
point out that as in the case of multiperson games, computer simulations
or one-person games also involve an element of human variability, and this
element complicates the analysis of their outputs and restricts the objec-
tive that can be achieved.

One-person games also admit of experimentation dealing with the
people who play them, although, apart from the training and selection of
company executives, this aspect of gaming is not well developed. To illus-
trate the type of purpose to which this refers, return to the air-battle model.
Once one is clearly convinced that this model satisfactorily simulates the
many complex interactions of a large-scale battle between strategic forces,
then he is in a position to use it for training or to attempt to test the capa-
bilities of various personnel in performing the tasks represented by the
selection of input parameters. Such tasks may be analogous to planning
an intercontinental war. It is not known whether this model has been
finalized to the point that one could place the great confidence in its re-
sults that the use of these results as criteria in the selection of planners
would require; however, there are games such as that played by some
members of the American Management Association (21) that serve simi-
lar purposes.

Multiperson games. For the more comprehensive games--compre-
hensive in that they involve several teams and hence the added complica-
tion of human interaction--one finds that the purposes toward which they
are directed are fairly straightforward extrapolations of those to which

one-person games are applied.* For example, two-person war games, which obviously admit consideration of the dynamic interplay between groups whose interests in the military situations may be directly opposed, are used in situations where dynamics and competition are important. However, these games are criticized by proponents of one-person games, particularly computer simulations, because the added degree of freedom and the complications induced by introducing additional human elements produce a situation in which analysis becomes much more difficult. The merit of these points is further examined in later sections, but it is important to reco; 'ze here that a multiperson game may have a purpose that is identica! .: statement to that of a simulation, but the context is necessarily broader, more problems must be considered, and decisions are made in a somewhat different manner.

In multiperson games, decisions are normally made at intervals throughout the play, and since they are made separately by two or more teams, information flow and control become important. These factors add new dimensions to the purposes normally found in one-person games, for not only can the same problems be examined in this broader dynamic context and new problems in which the interaction between groups is important be explored, but experimentation in interpersonal and intergroup behavior now becomes a possibility. Techniques for this type of gaming are not yet well developed; however, pioneering efforts have been undertaken at the RAND Corporation in its public opinion games (11) and in a number of other centers of research, most notably, perhaps, at the System Development Corporation.

Perhaps the most notable trends toward increases in context lie in the incorporation of dynamic economic and political factors in traditional war games. Th   rt also has received great impetus in this country through work at the .   ) Corporation (20), although there is some evidence that such combined gaming was used by both the Japanese and Germans in preparation for World War II. Traditionally, political factors are held constant during the play of a war game, and, conversely, initial political games tend to eliminate or treat in a very cursory manner the military factor in political crises (10). Gaming groups in both areas have come to recognize the importance of the interplay between military and nonmilitary factors; this recognition has resulted in games in which both factors are more realistically portrayed, and hence in games suited to the treatment of much broader problems (9, 22, 43). Yet even these games have basic structural elements that are common to one-person games, and many functions to be performed are common to both.

Predevelopment Analysis

Predevelopment analysis has often been accomplished at some level of detail prior to the establishment of a group whose job is to develop a structure and make preparations necessary for the conduct of a game. But it is often true that the gaming group finds it necessary to devote considerable time to a preliminary analysis of problems to be studied. Gaming groups often tend to inherit problems that have been analyzed by nongaming

---

*The SYNTAC war game conducted at the U. S. Continental Army Command is an example (12).

groups, with the results of this analysis found to be inadequate. The reasons for inadequacy are legion, but problems turned over to gaming groups most often are inadequately analyzed simply because there are interactions that previous groups have been unable to simulate or investigate. These most often involve human factors, or man-machine interactions. Problems in this form tend to be structured to varying degrees, for it is necessary to analyze a complex problem to some extent in order to conc' ide that there are interactions that are not well enough understood to be treated analytically. The problem may yield to gaming techniques, or gaming may develop sufficient understanding so that the problem will yield to subsequent analytic treatment. But the gaming group is well ahead if there has been a logical analysis of the variables present in the situation to be simulated, if the probable interactions have been specified, and if a logical structure has been developed. If these things have not been accomplished, then they may become tasks of the gaming group in their initial analysis of the problem.

The gaming group must be as certain as it can that all essential variables are included in the final structure developed, and it must define and later simulate the interactions believed to be important. This introduces a degree of variability in gaming models of identical systems and situations, and the judgment brought into play is often cited as reason for rejecting the results obtained. To avoid such criticism, or more positively to assure that the best judgment available is brought into play at this point in gaming, the composition of the gaming group deserves detailed attention.

Military games have sought to take advantage of the experience of senior military personnel and technicians who specialize in game development. The latter may assist in identifying essential variables and interactions, in the development of models (often abstract mathematical models) that will simulate them, in the preparations of rules of procedure, etc., and in the preparation of necessary communication and display systems. However, since one of the principal variables in most games is the human variable (decisions), it is clear that gaming groups should have access to persons skilled in behavioral sciences and experimental design. The criticism that gaming groups often do not contain behavioral scientists and that even consultation with such scientists is inadequate is unfortunately very valid. Also valid is the criticism that the introduction of individual personalities into a problem that has been treated analytically overly complicates matters and reduces the confidence that can be placed in the results obtained. Is this a necessary outcome however? In most cases I doubt it, or at least have doubts about it.

If one is able to establish with reasonable certainty that it is necessary to simulate human factors in order to gain greater insight into the processes involved in the operation under study, to evaluate the set of equipment being proposed, to aid in the development of tactics for the use of new equipment and weapons, to aid in exploring the implications of a new national strategy, or to seek any of the many aims for which games are being played, then one has arrived at the point where the interactions between individuals, groups, or men and machines appear to assume importance. Whether this importance is real or imagined is considered in the predevelopment analysis by the game preparation group and reassessed in the postgame analysis. Should this group be optimally composed, i.e., reflect the knowledge possessed by sociologists, experimental psychologists, etc., it is not difficult to conceive of situations in which the

knowledge gained in the expanded context must be regarded as being supe-
rior to that obtained in an analysis that ignores or subordinates the human
variables. Each situation must be examined on its own merits, but the
added knowledge gained by gaming may be negative in that it tends to say
that the human factors were not as important as previously supposed, per-
haps because the human responses were adequately predicted or because
proper steps were being taken to minimize or take into account the effects
of human variability. Yet even this negative knowledge is valuable and
provides a better basis for the evaluations being undertaken than uncer-
tainty relative to human factors. If this result can be determined in the
predevelopment analysis, the gaming group will have performed a very
valuable service.

In discussing predevelopment analysis the point is perhaps belabored
that in cases in which human decision, or human behavior, is one of the
essential variables in the structure of the problem, behavioral scientists
appear to be integral to a well-balanced game-development group. How-
ever, there appears to be inadequate appreciation of the need for participa-
tion by such scientists in all phases of game development and play. Most
prominent is a failure to fully appreciate the fact that placing individuals
in a game situation introduces psychological factors into the simulated
situation that may not be relevant to the operation being simulated, or may
in fact be detrimental to it, and that the control of these factors needs to be
considered in detail by the game-preparation group, beginning with the pre-
development analysis and continuing throughout the game development and
play. Given adequate consideration of this kind, it would appear possible
to minimize the effect of extraneous psychological factors, or at least to
control them to the point where an analysis involving human beings would
be clearly superior to one that ignores human factors in cases in which
the variability in human behavior can easily be seen to have a significant
influence on the problems under study.

In summary, then, it is pointed out that, though predevelopment analy-
sis may be formal or informal, it is absolutely necessary in order that
the essential variables be defined, that their interactions be understood
and that a clear logic be developed for the simulation. It is clearly desir-
able to take full advantage of whatever experience is available. One of
these variables may be the behavior of various individuals in the situation
being simulated, and when this is true the inclusion of behavioral scien-
tists in the game-preparation group, or their use as consultants to it, is
highly recommended. But even when this is not the situation, the placing
of individuals in a gaming environment generates psychological forces that
require consideration and control throughout the entire process associated
with gaming. The degree of control exerts significant influence on the
utility of the individual game either for training or research, or for the
many other purposes to which games may be put.

Establishment of Procedures and Rules for Play

If the pregame developmental analysis has been completed, one has
prepared a flow chart or diagram that identifies essential variables and
interactions and that presents the logic underlying the system being simu-
lated. (See Davy (12) and Weiner (43) for typical examples of flow charts.)
To some extent the game-preparation group will have determined the num-
ber of teams required, the major decisions that they will make, and hence

the broad requirements for assessment and even perhaps the type of analysis of results that will be needed. However, detailed requirements for assessment and postgame analysis cannot be finalized until the procedures and rules of play are examined.* The decisions made in this examination are governed by the purpose of the game; they are also determined to a limited extent by the utility of the game for various purposes.

To be decided at this time are such important factors as the type of decisions to be made and the methods by which they will be arrived at, the form in which they will be prepared, the manner in which they will be monitored or controlled and the results of these decisions assessed, the time between decisions and the manner in which the actions in the intervening period will be handled, the types of communications to be provided, the physical facilities required (when these have not constrained the planning up to this point), the level of aggregation or detail to be preserved (when this has not been already agreed on or specified in the terms of reference for the game), and a host of procedures for handling the many administrative details of game operation. Many of these decisions will severely restrict the individual problems that may be treated by the structure to be developed; others will indirectly affect only relatively few problems and have little bearing on the ultimate utility of the game as a research procedure. Only some of these functions are illustrated or elaborated on below.

In the one-person game most of the decisions required of the game-preparation team are relatively easy to make, but only relatively so, because much depends on the nature of the problem and the scope of activity being simulated. In some of the analog simulations of the Lanchester equations, for example, there are relatively few parameters whose values have to be specified, and this can be done by a single player.**

In this form, the Lanchester model of combat is completely deterministic, and hence once the input parameters have been specified the resulting end states and time-dependent losses can be obtained through calculations that can be performed with acceptable accuracy on an analog computer. Apart from the computer and display mechanisms, few physical facilities are required, and since no teams are involved, problems associated with communication do not arise. The problems of how the few decisions to be made are to be arrived at and how the results are to be analyzed and interpreted remain, and if one is seeking to apply this model to actual military forces, they are difficult.

---

*Thompson (41) presents a set of rules for a modified division-sized combat force emphasizing (as did the INDIGO game of the Operations Research Office) the relation between units and the need for communications. These rules are in a formative stage, and their document indicates this phase of game development quite well.

**The equation may be written as

$$dy_i/dt = -\sum_j e_{ji} y_j; \quad i = 1, \ldots n$$

where y_i is the number of units of type i available at time t, and e_{ji} is the rate at which a single unit of type j destroys a unit of type i. These equations define the rate of losses on both sides.

The Lanchester model has been used extensively in the study of combat interactions, particularly between two opposing forces of similar or directly opposed types, and simulations of it that have been placed on a computer are not usually referred to as games. However, if used to examine actual or potential combat between specific forces, the simulations fit into the category of games as defined herein, for there is a single set of decisions that must be made at the beginning of play and these establish a correspondence between an actual system and a hypothetical one. Further, the results of these decisions are then assessed, in this case by a completely deterministic mathematical model; when inferences are drawn from the results obtained, the gaming cycle is complete.

In multiperson games the decisions made in establishing the format of the game are much more difficult and have to be carefully related to the purposes of the proposed game. This is illustrated by considering a two-person game in which NATO forces in Western Europe engage Soviet forces, although the points relating to significance apply equally in most multiperson games. The range of problems that possibly could be considered in this game is extremely large and is narrowed usually by determining the degree of resolution of the game.

Resolution is discussed in detail later, but it usually relates to the size of unit, the size of the battle front, the degree of detail to be placed in terrain studies, etc., that are agreed on. It differs greatly according to the aspects of combat that are emphasized. One game may be developed to study equipment associated with infantry companies, another with submarine squadrons, and another to examine the aspects of strategy related to possible use of nuclear weapons. Consideration of company actions is entirely too detailed in a study devoted to the effects of nuclear weapons usage across a front of the size now existing in Europe; but division-level resolution is clearly too gross for the study of company equipment. Thus some of the principal decisions to be made in this phase of game development, if not prior to it, relate to resolution or degree of detail.

Other important decisions that have to be made in this phase relate to the types of decision to be made in the game, the manner in which the decisions are arrived at, and the methods and control of communications.* The types of decisions are closely related to the level of resolution and game purpose, of course, because decisions to move a division are important in games whose resolution is at the division level or higher, but provide only background information in company-level games. The content of such decisions, the manner in which they will be recorded, the class of acceptable decisions, and the way in which decisions will be reviewed to assure acceptability--whether they will be transmitted to an umpiring group or be monitored through computer techniques--are all problems to be treated in developing rules and procedures; the manner in which they are arrived at must also be decided, and this determines team structure to a large extent.

There is also a series of administrative decisions that bear on the validity of the simulation, and hence on the utility of the game; these relate

---

*See Davison (11) and Weiner (43) for discussions of the role of communication in games.

to physical facilities, their equipment, and layout. Games have often been criticized because the facilities were such that simulation was impeded. Spaces have been provided for player teams that were too close together and lacked adequate privacy; this has contributed to situations in which player moves were difficult to monitor and the time elements in the simulation were unduly distorted. Frequently the facilities have lacked many of the normal accouterments associated with the echelon of command being simulated: maps, aides, daily staff resumes, and appropriate communications between echelons simulated. These lacks seriously impeded the formulation of command decisions that were representative of those to be encountered in typical field operations. All such factors must be decided in establishing the rules of play, and because of their complexity it is not unexpected that games have become dynamic in development, with rules being modified during plays and between plays of similar situations.

As a final important element to be considered in this phase, there is the element of time. Simulations invariably seek to reduce the time involved in actual play to the bare minimum, a course of action that is necessitated by the requirement for experienced and highly skilled personnel as players. However, the decisions (a) fixing length of the period to be simulated, (b) determining the intervals at which decisions are to be made, and (c) choosing between hand and computer assessment are fundamental. The length of the period simulated defines the gross nature of the assessment model, whereas the intervals at which the situation is reviewed not only determine the detailed characteristics of the assessment models but also define to some extent the scope of problems that can be properly examined. A long review interval requires the exercise of great skill in continuing the actions occurring in the situation in the intervening time, but an interval that is short relative to the game interval requires many intermediate reviews and hence lengthens the player time required. On the other hand, a decision to assess by hand implies a very long assessment period for even a short interval. Thus one must adjust the decision intervals to the method of computation and to the total time simulated. In no place in gaming does the element of compromise enter so fully as in the area of time; however, the type of assessment and the type of models used in assessment tend to dominate time considerations in their influence on game development.

Establishment of Models or Rules for Assessment

The assessment of the outcome of decisions made at varying intervals throughout the game is a function that is performed with varying degrees of objectivity in both one-person and multiperson games.

In the typical one-person game the actual assessment of outcome tends to involve a minimum of subjective judgment as has been indicated, but the familiar process of determining essential variables and their interaction is subjective to a large degree whether one attempts to develop a set of simple qualitative rules for determining the outcome of decisions or a highly abstract and sophisticated mathematical model of the interactions taking place. The latter is very common in the simulation of some aspects of combat interactions, or of combat movement or traffic flow, but the results obtained still contain a subjective element and one that can be eliminated only on the basis of experimental data or, in some cases, historical data (22). Validation of the results obtained is often easier for highly

abstract one-person games, but here also, in the absence of actual performance data, one is forced to rely on judgment and such limited experience as may exist for the validation of assessment models. This is true regardless of whether they are analytic and highly abstract or qualitative and relatively simple. Yet the validity of the assessment models is a basic element in determining the acceptability of results obtained.

In multiperson games the methods of assessment may simply rely on the judgment of experienced personnel; more often attempts are made to incorporate assessment models from one-person games, or computer simulations are incorporated into the assessment procedure.* Rules are established that serve as guides for umpires charged with the assessment functions that cannot be otherwise made (see 4 for an illustrative manual of rules). The more complex the operation being simulated or the greater the lack of experience in the actual performance of the operation, the more one is forced to rely on the judgment of personnel having limited experience in the problem being studied or experience in related areas. This is as it should be, for it is only after interactions are reasonably well understood that one can develop acceptable rules to guide assessment or accomplish the often more difficult job of constructing a reasonably precise analytical model of the situation. However, such preparation usually continues as plays are made and, as is discussed later, multiperson games are often effectively used in continuing cyclical studies of very complex problems, with assessment models becoming more abstract and analytical with each play.

in summing some of the considerations pertaining to assessment models that are relevant to an evaluation of gaming techniques as research procedures, it is seen that in both one-person and multiperson games assessment models contain elements of subjectivity, the significance of which can be reduced if either historical or experimental data are available for model validation. In order of decreasing subjectivity, assessments are made by (a) experienced personnel, (b) a set of rules that may be continually modified and improved as the game is repeated, and (c) analytic models whose validity may be improved through the use of historical and experimental data. One-person games usually employ methods (b) or (c)—most often (c)—and hence tend to have imbedded in them a lesser degree of subjectivity. However, individual games vary so much in the quality of their assessment models, that generalizations are hazardous.

Preparation of Scenarios for Individual Plays

Although some games have been developed solely to consider a single situation, it is more usual that a gaming structure is developed so that a wide range of situations can be examined in turn; hence there are a variety of preparations that have to be made for each individual play. To a large extent these parallel the functions previously described, although the emphasis here is on review and modification to meet the particular problem undertaken. There are, in addition, the functions of data preparation and scenario writing that are somewhat unique to this phase.

---

*See Helmer (23) for a discussion of the relative merits of rigid rules and umpire rules and also of machine play and human players.

In revising previous work the establishment of purpose is made more
detailed and precise. A preliminary analysis of the particular problem at
hand, although often not absolutely necessary, is helpful i.   ...ing com-
patibility of the gaming structure and the particular problem. The struc-
ture developed to data may not have emphasized the principal factors of the
problem under study and hence may require addition and modification. The
procedures and rules for play may also require expansion or modification
to improve the focus of the game. The assessment models in particular
may require substantial modification so that greater detail is incorporated
into those aspects emphasized in the current problem. But since these, as
well as models that require no modification, may be quite abstract there is
a substantial problem of data preparation to be faced.

Data preparation. The collection, evaluation, extrapolation, and re-
cording of data can be a formidable task requiring the skills of senior sci-
entists of many types. It may require the specification of the order of
10,000 constants, as in the CARMONETTE computer simulation (Adams,
in 33), or the securing, organization, evaluation, etc., of background mate-
rials for 16 countries as was the case in the SIERRA games at RAND (43).
In one such SIERRA game each of the 16 countries was discussed under a
series of subheadings: history, geography, people, religion, type of gov-
ernment, political background, economy, resources, external policies and
politics, and armed forces. There are few social science areas that do not
mesh into at least one of these categories, and the preparation of valid,
relevant data can clearly become an enormous task.

Scenarios. Individual plays are essentially different; hence a scenario
is required for each situation. Scenarios may be written by various mem-
bers of the game-development group or by this group with the assistance
of experienced and probably extremely busy personnel that are available
only for this function (1, 5). Regardless of how it is prepared the scenario
is an extremely important aspect of the game, and its construction differs
according to the time of occurrence of the event to be examined.

When the time frame of the game is in the present or past the scenario
is generally a historical document describing events leading up to the situ-
ation to be examined. This description attempts to cover all aspects that
appear to be important relative to the decisions to be examined; hence the
element of subjectivity enters again. If all players are very familiar with
the situation, the scenario can be very short, and the subjective element
is perhaps minimal; however, when this is not the case, great skill is re-
quired to extract the important events leading up to a crisis that the game
may seek to explore. The selections made at this stage limit the factors
to be considered, and therefore expert historical assistance is often
required.

Many military applications of games involve new weapon systems or
emerging tactics or strategies and hence are relevant to future time pe-
riods (37); this poses severe problems for the scenario writers and some-
what limits the applicability of any detailed results obtained. In this case
one must extrapolate history, and the elements of intelligence and judg-
ment come very much to the fore. The scenario must characterize a set
of conditions that is likely to exist in the future, and it must communicate
this characterization to the prospective players with sufficient clarity to
permit them to start from a common basis and to make reasonable decisions

about hypothetical activities. But how does one characterize a set of conditions associated with such complex situations as are simulated in typical military war games, and what skills are needed if the characterization is to be satisfactorily completed? The answers to both questions vary greatly with the conditions to be simulated, but those involved in typical war games concern those aspects of culture that lead to the most violent of conflicts. The characterization of culture involves skills normally possessed by anthropologists, economists, sociologists, etc.; but the characterization of military conditions involves another range of military skills as well. These facts indicate a team approach to scenario writing—and one containing appropriate social science skills.

Even when attempted by appropriately composed teams, the characterization of conditions leading to a war—even for the restricted purposes to which many war games are directed—can be an extremely difficult job. Obviously, the extent to which this can be done effectively depends on the remoteness of the time being studied and the detail involved in the problem at hand.

The problem may seem tractable if only one, two, or three years are to elapse before the postulated event occurs, but even here great uncertainty is introduced when political decisions and human factors must be included.

For this length of time, military plans and weapon systems are generally relatively firm, for it is often said that lead time for the average major weapon system is far in excess of three years, i.e., in reference to time of troop delivery. Archer (1) assesses lead time for new military systems at 5 to 10 years. And to the lead time must often be added a period of training and adaptation, so that a 3- to 4-year extrapolation is reasonable for military organizations and their equipment. Political factors have much shorter lead times and are far less certain. Individuals often seem completely to change their outlook without obvious warning. Thus games that treat problems that are highly sensitive to the results of future political decisions or individual personalities must be evaluated with rapidly diminishing confidence.

This diminishing confidence is often not as serious as it may appear, however, for if games are to affect actual decisions relating to military actions or equipment in distant time periods, they need not be detailed and cannot affect actual postures without frequent and more detailed subsequent reviews. Thus the scenarios for the more distant period can be more generalized, and must of necessity be so, and still correspond to the scope, complexity, and definitiveness of the decisions or trends that are most often taken as subjects to be investigated.

Scenarios for one-person games are usually not very formal, or, if they are, they are not generally referred to by that term; however, the same purposes are served in informal instructions for playing the game or with descriptions of the abstract situations to which the somewhat more abstract models apply. Generally treating problems that are smaller in scope, many variables are suppressed in order that other variables, presumably the more important ones, may be subjected to detailed scrutiny. In these cases, interactions are assumed to be fairly well understood, and the characterization of pre-conditions of play can more easily be made complete and is more readily communicated.

In summarizing the aspects of scenarios that bear on the utility of gaming as a research procedure, it should be noted that the relation of purpose to time frame, scope, and the skill and experience of persons preparing the scenario is important. The degree of skill exercised in abstracting from the mass of detail that can be presented those aspects that are important enough to deserve treatment in the scenario will determine, in part at least, the emphasis and consistency of the play, and hence the quality of research that might be accomplished. Historical and other social science skills are likely to be quite useful in this phase of gaming (43).

Selection of Players and Formation of Teams

No functions in the gaming cycle have a more direct influence on the outcomes to be achieved than those of player selection. This is true of both one-person and multiperson games.

One-person player selection. In the one-person game many functions coincide, and only a single player or a single team of players is required; however, the single player or team must attempt to relate the usually abstract variables associated with the model to the real world in such a way as to make the results obtained meaningful. Even in as simple a game as one based on the Lanchester equations with two variables, this may involve a considerable quantity of analytic study if the appropriate input parameters are to be properly defined. The quality of the personnel assigned the task of parameter definition will greatly influence the validity of the results obtained.

This point seems sufficiently important to deserve illustration, which is done by reviewing a potential application of this model to the analysis of the effectiveness of tank armor and firepower. If one assumes that the primary function of tanks in armored units is to fight tanks, that tank fire in battles can be distributed so that the average rate of kill of the tanks on each side remains constant throughout the engagement, and that a few other simplifying assertions hold, then one can attempt to simulate tank-vs.-tank combat through the use of the equations

$$dy_1/dt = -e_2 y_2, \qquad dy_2/dt = -e_1 y_1$$

where the symbols are as defined earlier (page 200).* Of course these equations greatly oversimplify the interaction between tanks on the battlefield, and no attempt is made to support their validity; but they do serve to illustrate the importance of player selection.**

The differential equations can easily be solved through electric analogs that integrate them as a function of time; hence both the time decay of forces and the end states of hypothetical battles could be displayed on output mechanisms, say on a cathode-ray tube with appropriate grids. If this were done, one would have established the physical requirements for a simple one-person game, one in which there would be only a few decisions to be made. The constants e_1 and e_2 would have to be specified—a decision

_____

*See Davy (12) for an application of these equations to infantry combat.
**See (2) for further discussions of tank-vs.-tank and tank-vs.-antitank gun simulations.

as to the initial strengths of the forces and a decision as to when the battle would end in the rout of one of the two forces. These decisions could be varied over a wide range of values to determine the basic characteristics of the model itself. This could be accomplished by an analyst with moderate mathematical skill, but if the model were to be used to investigage some of the properties of tanks, the qualifications of the "person" immediately increase and the formation of a team is suggested; for it would be necessary to extract from all the characteristics of tanks that influence firepower and armored protection, those that are really significant, and somehow relate these to the effectiveness constants c_1 and c_2. This is not a simple problem and a priori one could not say that the model could be applied with adequate validity.

To attempt to determine whether this were in fact possible, a study might be made of the history of tank-vs.-tank fighting in which opposing forces used different types of tank. The purpose of this study would be (a) to determine whether reasonably consistent values of the constants c_i could be obtained for tanks whose characteristics were well known, assuming that the equations were indeed applicable and (b) to gain knowledge about the conditions under which engagements ended. Assuming some encouragement from this endeavor, the relations between gun size, armor thickness, reliability, weight, logistic requirements, mobility, tank size, crew behavior, etc., and military situations might be examined. This examination would obviously require skill of types different from those used in the initial effort, and hence different personnel. It would focus on tanks for which combat data were available as well as on those under investigation. The results might be quite revealing and in themselves instructive and valuable, but they might not provide a method for determining the constants c_i or they might cast doubt on the utility of the basic equations.

One could conceivably find as a result of this type of pregame analysis that the model being constructed, although correct in its basic structure, was entirely too simple. It might turn out, for example, that the constants c_i could be approximated by analytic functions of the parameters investigated and that these functions appeared to agree with experience. This agreement could be uncertain enough so that confirmation by experiment and/or refinement with the assistance of persons quite skilled in tank design, human engineering, and use were urgently desired. As a result one could conceive of a modified and expanded Lanchester-type model in which the constants c_i were replaced by complicated functions of basic tank parameters and a player group selected to exercise the model so as to comment on its authenticity. Clearly the selection of this player group would be an important task. It might not assume equal importance in every game played, but it is easily seen that the selection is always important, and, depending on purpose, team composition may dominate other factors in determining the quality of research achieved.

Players and teams in multiperson games. As the multiperson category of games is examined, it is found that the importance of team composition and player selection does not appear to decrease, although the selection problems become more complex because of the interpersonal problems that arise in intergroup activity. The most typical multiperson military game involves three teams—two teams whose interests are somewhat, if not totally, in opposition and an umpiring team.

The functions performed by the umpire team, or "nature" team as it is often called, include the monitoring of player decisions, assessment of the results of these decisions, updating of the military situation between plays, injection into a game of the results of decisions that normally would be made by groups other than those given explicit team representation, monitoring of intelligence activities of the two teams, maintenance of game records, and a host of other activities that are necessary to keep the game going and to direct its activity toward constructive ends. Games may be played on schedules that are flexible with respect to the interval between decisions or to the time allowed for decisions, and the umpire or nature team must continually decide these matters. The time that the umpire group can devote to this depends on the arrangements for other functions, particularly those of assessment and record keeping.

It is worth noting that computers have been used to perform many of the functions of the umpire team—significantly record keeping and assessment. In some instances computers also provide communications systems. The use of computers decreases player requirements and relieves highly-skilled personnel of routine duties that are normally performed by persons of lesser skill and rank.

Decision-making teams also need to be relieved of these routine activities and if possible to be provided with preliminary assessments of the results of potential decisions. To obtain these estimates they will of course draw heavily on such experience as they may have, but when complicated military actions are involved, persons having to make such actions normally have very large staffs to examine the many ramifications, such as:

(a) How adequate are the available supplies of various types to complete a desired maneuver?

(b) Is the knowledge about current enemy disposition adequate, and what do certain intelligence items imply about possible changes?

(c) If a certain unit is ordered to engage an enemy unit, when could it enter the conflict, and what bearing would that have on particular outcomes?

(d) What are the relative values of enemy cities, and how does the allocation of aircraft to targets near them increase the probable enemy civilian casualties?

Information relative to factors such as these can be stored in computers or can be computed if proper routines are available and made available on request; thus the staff requirements for the conduct of play can be measurably reduced and the size of player teams brought into regions where repeated plays become more possible. In highly abstract and greatly aggregated games, similar computer use permits greater attention to be given to factors affecting the outcome of the decisions or the problems under study.

No attempt has been made to cover at length many of the important aspects of player selection and team formation; in particular, it has not been pointed out that the number of teams may vary widely (as many as 10 teams being established in the multination political simulation played at

Northwestern University; see 22) and that the problems associated with information storage, player selection, and interpersonal and intergroup activity increase greatly as the number of teams increase. However, it appears that enough of the significant problems in this area have been covered to give the reader an awareness of the fact that the utility of a game as a research technique critically depends on player selection, team formation, and the handling of a large number of psychological variables associated with gaming personnel. So critical is this dependence that it is difficult, if not impossible, to dissociate the results of games from the people who played them.

Play of the Game*

The play of a game is usually preceded by a period of indoctrination for players that may vary greatly in length and intensity of activity. The scenarios must be thoroughly comprehended, the game structure fully appreciated, the assessment procedures understood, and a variety of other factors examined in order that the players might intelligently participate in the simulated activities. Practice in playing games is a great advantage, although experienced players are not always available. However, previous games have shown that players generally enter into the activities of gaming with enthusiasm, and learning proceeds apace. In most instances even the skeptics appear to have found the activities both instructive and useful; this has been especially true of experience with competitive multiperson games perhaps more than with computer simulations or one-person games (9).

In the one-person game, competition exists only in the sense that an individual player may seek to establish results that are superior to those of other players. Thus the element of competition is somewhat remote, and the stimulus offered to creative activity by interpersonal contact and intergroup activity is essentially lost.

In multiperson games, on the other hand, the play is often characterized by an intense competitive spirit generated by dynamic activity between groups whose interests are at least partly opposed. The intense rivalry that arises sometimes interferes with game objectives and causes players to "play the rules" rather than continue the simulation faithfully. The resulting emotional intensity can generate problems of serious magnitude for the umpire or control team, especially when assessments are based on loosely defined rules or judgment alone.

Umpires are often faced with situations in which the course of events could eas. take several turns, depending (a) on events that are not under control of the players or (b) on those within their control that have not been specified in adequate detail to make assessments unambiguous. The umpires may request clarification, thereby alerting the team to the sensitivity of the action taken and to the probable action taken by the opponents, or they may make decisions that tend to direct activities along courses that permit more detailed exploration of the problems posed by the game directives. In either circumstance it is customary for the umpire team to

---

*See Weiner (43) for discussion of the mechanism and techniques of play.

note that a critical point, or in gaming parlance what is more often called
a branch point, was reached in the game at that time. In fact, as in some
of the games played at the RAND Corporation, the determination of these
branch points may be the purpose for which the game is played, and once
these are determined the effects of alternative decisions may be explored
at a later time, often through additional gaming (14).

In exploring some of the aspects related to the play of the game, the
focus has been on player motivation generated through interpersonal and
intergroup activity and on the role of the umpire team in guiding and con-
trolling the actual play. This has been done not because these are the only
features of game play that are significant with respect to gaming as a re-
search procedure but because they are significant and also because they
tend to distinguish multiperson games from one-person games in general
and certainly to distinguish gaming analysis from the more analytic or
rigorous types of study.

Postgame Analysis

The desirability of planning the postgame analysis prior to the estab-
lishment of rules or models for assessment has been indicated earlier; it
suffices therefore to indicate some of the typical methods of postgame
analysis and to comment briefly on their usage.

In one-person games in which assess   · are made through quanti-
tative analytic models (or through carefull   . ned sets of rules) it is
generally possible to obtain vast quantities .. data, depending of course on
the detail incorporated into the models and the arrangements for record
keeping. These data can be processed through standard statistical tech-
niques or through the use of the many mathematical techniques of analy-
sis available. The various measures of dispersion and central tendency
are often used, although correlation techniques, factor analysis, etc., are
being used more often (see Wagner, in 33). As computer simulations have
increased in complexity it has been found desirable to treat them with ex-
perimental-design techniques in order to explore the sensitivity of param-
eters and to make the most of the data generated. The same techniques
are applicable in some degree to multiperson gaming analysis, depending
on the extent to which the rules are quantifiable.

In the multiperson game, however, typical modes of postgame analysis
may be aided by questionnaires, conference, critical reviews of game rec-
ords, and group discussion. In discussion sessions devoted to this activity,
opposing teams are able to abstract some general principles that the game
seemed to develop and, from the clarification of each other's intentions and
strategies and the gaining of information, to complete their knowledge of
game activities, to point out problems that appeared to be critical and de-
serve more intensive study, and generally to arrive at some measure of
agreement on broad conclusions related to the activities under study. Re-
visions in rules and procedures are generally suggested at these sessions
or in follow-up questionnaires of analysis checklists calling for reactions
to the game after a period of reflection.

The records of the game provide a basis for content analysis, some
types of historical and psychological analyses, extraction of data for treat-
ment by statistical techniques to establish measures of interaction,

requirements, correlations, etc., and othe. results. Other techniques are
also used to evaluate the proceedings of multi_   ~n ~ames, but those dis-
cussed suffice to give the reader an appreciatic > _. the scope of activities
that take place in this phase of gaming.

UTILITY OF GAMING AS A RESEARCH TECHNIQUE

The utility of gaming as a research technique, i.e., its usefulness as a
method or way of performing the mechanics of "carefully hunting for truth
of facts," has been brought into question before; not only has this question
been raised by many scholars,* but it was the topic of greatest interest at
the recent (1961) meeting of United States, United Kingdom, and Canadian
representatives to the ABC Conference on Army Operations Research. At
the annual meetings of the Symposium on Gaming conducted by the Univer-
sity of Michigan, this question has invariably been considered. It was also
a matter of central concern at the First War Games Symposium of the
Washington Operations Research Council in November 1961. Why the pro-
tracted interest in this question in recent years?

Although gaming as a military activity dates back to antiquity, its use
as a formal research tool, especially in the functional form previously de-
fined, appears to be a product of the twentieth century, and perhaps of the
period following World War II (37). This usage has increased as opera-
tions research has developed, and this is at least partly due to the increas-
ing complexity of the problems that operations research has sought to
solve. Expanded usage, whether for research or for training, was prob-
ably inevitable, however, for with a rapidly expanding technology the com-
plexity of military problems also increased. As the complexity of military
problems grew, older methods of analysis proved inadequate, and both the
military and their scientific associates, the operations analysts, turned
to gaming to gain at least a better understanding of the problems to be
solved and of the complex interactions of the increased numbers of de-
pendent variables (1). Yet some of these complex problems have not
yielded to this treatment. Since gaming is a very expensive form of anal-
ysis, it was inevitable that the utility of the techniques would be subjected
to increasingly penetrating scrutiny.

This discussion considers gaming both as an isolated technique and
as a part of more complicated processes; it is in the latter role that the
use of multiperson gaming has become so widespread. However, in prepa-
ration for these discussions it is necessary to enlarge somewhat on previ-
ous discussions of the scope of gaming activities and to indicate some of
the limitations and objectives that appear to be obtainable through its use.

Scope of Gaming Activities

At the Third Symposium on War Gaming at the University of Michigan,
a scheduled speaker said that gaming was being used in the solution of
military problems from the level of the squad through those of the field

---

*See Smith and Marney (38) for a discussion of gaming as a concep-
tual cognitive model and for an iteration of scholars' questioning its
applicability.

commander, the Joint Chiefs of Staff, and the nation.* This is a wide
range indeed, and it suggests that the problems covered must differ in de-
tail, in degree of aggregation, and in resolution by large factors.

At the squad level the principal concerns are about the individual
soldier—his equipment and his tactics—and about squad equipment and tac-
tics. There is no reasonable division of the squad, which contains on the
order of a dozen men, into many units. Thus a squad game might be con-
cerned with teams containing several men each, but its lowest resolution
would most likely be that of an individual soldier.

At this lower level of resolution, games treating company units are
more common, and often these tend to be computer simulations (see Ad-
ams, in 33, for an example). Tactical weapon systems characteristics
of individual pieces of company equipment, etc., have also been treated at
this level, and the most significant difference between company- and
squad-level games appears to reside in the treatment of terrain. At the
lower level, the degree of movement is not great, but the space occupied
and covered during the execution of combat functions is greatly increased
when company-level activity is reached, and games that do not incorporate
terrain effects in some way tend to become highly unrealistic.

As unit size increases and the theater-level game is reached, the
complexity reaches the point where company-level resolution is most
often impracticable and division-level resolution may be desirable. Spe-
cial smaller-sized units that have great combat potential may also be in-
cluded as separate entities, but the necessity for considering joint (tri-
service) or even combined (alliance) operations, intratheater and some-
times intertheater logistics, makes it more evident that economic and
political factors have assumed greater importance. Games of the type
that seek to include dynamic political and economic factors to a degree
have evolved at RAND and the Research Analysis Corporation, and in both
cases it was found necessary to draw heavily on the contributions of social
scientists—particularly historians, economists, and political scientists (43).

At higher echelons—Army, Navy, Air Force, Joint Chiefs of Staff, and
National Security Council level—the scope of the activity grows, and so
does appropriate unit size (39). Questions of balance of effort, both within
military specialties and between military and nonmilitary activities of var-
ious types, become important and must be included in any gaming activities.
Insofar as the topics under consideration at these high levels involve rela-
tions with allies and with enemy or nonaligned nations having cultures
vastly different from that of the United States, many different aspects of
social science assume greater importance (29). These are evident in such
games as the 10-nation simulation at Northwestern University (22), whose
purpose was to investigate relations between the political necessity of
satisfying internal demands for consumer and military items and inter-
national political requirements; or the political exercise POLEX II (9)
conducted by highly-skilled social and physical scientists and military ex-
perts to see whether political gaming could contribute to the development

_____

*Note that well-designed games suitable for play of limited-war
problems at the Joint Chiefs of Staff level were not believed to exist in
April 1961 (20), perhaps owing to emphasis and resolution in service games.

of foreign policy. Clearly the range of gaming activity has been very great, but in spite of this its advantages as a research technique are limited regardless of the level of game activity.

Gaming Limitations

Many of the gaming limitations have already been alluded to; however, it is desirable to attempt to summarize and elaborate the more important ones. The limitations discussed apply generally to individual games and with greater force to some games than to others, but they also tend to set broad boundaries to the objectives that can reasonably be expected from gaming as a research technique. Among the principal limitations, one must include the practical degree of resolution that can be obtained, the limited correspondence with reality that is achievable, the limitations on replicability, and frequent lack of validated input data, the extent of skill and experience of players available, and most often the lack of adequate means of independent verification of the results obtained. In discussing these factors, the gaming of a postulated war between NATO forces and the Warsaw Pact nations in Europe may be used as an example.

Resolution depends on the purposes of a game, on the factors emphasized in the construction of models, and in the development of game structure that results from such purposes. In the case of a European war, for example, one might attempt to study the functioning of a proposed Army logistic system. Because of the size of the forces deployed, it might be necessary to consider in detail only the major ports, airfields, depots, and means of transportation. Supply companies would certainly be too small to be considered. But the logistic system would have to function in conjunction with rapidly developing ground, air, and, possibly, naval actions. Thus combat models for these forces would probably be required. Also required would be communication models, intelligence models, etc. However, the intelligence models would be considerably less detailed because of the emphasis on Army logistics. Thus the initial model developed would not be well suited even to the study of other Army logistic problems, such as battalion supply problems, and it would be equally unsuited to the study of nonlogistic Army problems. It would probably be totally unsuited for detailed study of factors affecting air and naval forces.* In this way resolution tends to constrain the applicability of individual models and to set some limits to the degree of detail that can be considered through gaming techniques.

It should be pointed out in passing that the use of computer techniques and the continuous development of individual models to reflect different objectives have been extremely helpful in alleviating constraints imposed by resolution and in improving the realism associated with the model.

As with any type of simulation the correspondence with reality that can be achieved is always constrained, and this is particularly true of the

---

*See Weiner (43) for a discussion of the methodology used in the Project SIERRA war games played at the RAND Corporation and an example of limited-war gaming that included air, sea, ground, and logistic factors but whose emphasis in resolution was quite different from that in the example cited.

type of model developed for the study of military problems. In military
problems one is almost always concerned with events that have not yet
occurred and that cannot be tested in laboratory-type experiments nor
even completely tested in field experiments. As the time between wars
increases and newer weapons and weapon systems are introduced, the data
base on which realistic models can be devised decreases rapidly (1). Not
only are the precise effects of United States systems, the probable tactics
with which they will be employed, and the probable enemy reaction less
well determined, but also the knowledge of enemy weapon systems and tac-
tics tends to become even less complete. This problem has been greatly
accentuated since the development of nuclear weapons, especially tactical
weapons, for the effects of such weapons are not only qualitatively different
but of a different order of magnitude than those heretofore used in combat.
It is introduction of such weapons that has given much of the impetus to
gaming in the past decade, but, ironically, many of the difficulties assoc-
iated with correspondence to reality stem from the same source.*

Problems associated with the likelihood of the use of nuclear weapons,
say in the postulated European war, and the conditions under which they
will be introduced have precipitated much debate in gaming circles as to
the validity of many of the results obtained through traditional gaming
techniques. In past wars there have been few weapons, poison gas being
the notable exception, that have not been clearly usable under all circum-
stances of war. But with the possibility of escalation looming in the back-
ground of any future conflict that starts with conventional weapons, the
interplay between other political factors and strictly military considera-
tions assumes greater importance. This is evidenced by contextual stud-
ies at RAND (14), the explicit inclusion of military factors in political
exercises at the Massachusetts Institute of Technology (9), and by the com-
bined political-military games now being played at the Research Analysis
Corporation. The complexity of war gaming has clearly increased to the
point at which the outcomes of individual plays must be subjected to the
strictest scrutiny and at which continuous game development is a must.

It is generally accepted, especially, and perhaps surprisingl,, by
game preparation and evaluation groups, that the results of individual
games have limited validity so far as outcomes of possible future wars are
concerned. A single play really represents a simulation of one of the
many possible sequences of circumstances through which a war could start
and only—and indeed only—one set of tactics, one set of the many, many
possible outcomes of individual interactions and assessments. Thus one
cannot regard these results as being definitive or even necessarily typical
of the detailed result that might occur. So much depends on the skill with
which the eight functions previously discussed are performed that one
would normally use the results of other techniques if they appeared to be
adequate. And one would certainly seek to obtain additional plays of the
same or similar situations if that were possible. But with the complexity
of modern war, with the interplay between nonmilitary and military factors

---

*See Johnson and Wilson (25) and O'Neill (31) for an example of a
game investigating petroleum, oil, and lubricant requirements in nuclear
combat. Logistics under conditions of nuclear combat present an ex-
tremely difficult planning problem that has generated numerous computer
simulations and two-sided gaming exercises.

assuming greater importance, and with the significance of human behavior remaining a basic element in outcome assessment (and one not yet having been subjected to study adequate to permit easy quantification), alternative results are most often not readily available. Unfortunately the number of gaming replications that are feasible within resources now being allocated to gaming activities is definitely limited.

The inability of gaming group: to provide sufficient independent plays to provide statistically significant outcomes has long been recognized as one of the principal limitations of multiperson gaming, and considerable effort has been devoted to removing this limitation or to reducing its impact. These efforts have generally involved mechanization of the assessment models and bookkeeping procedures, increased facility in communications between player groups, and other such actions that either permit repeated probabilistic assessments of a set of game decisions or reduce the time and effort required to play an individual game. Although much progress has been made in both directions—one giving a spectrum of results from a single scenario and a single set of decisions, the other permitting more games to be played (but not orders of magnitude more)—until the ability to replicate is vastly improved, the utility of gaming results for many purposes continues to be severely constrained.

The limitations imposed by the skill of players are obvious; however, data requirements for games, especially those with more detailed resolution, and the resulting need for greater field experimentation deserve comment. (See 4, 16, and 26 for examples of data requirements.) Assessment models require some degree of quantification if the interactions of various numbers of military elements are to be studied. Typical responses to this need, as well as to desires to minimize the element of subjectivity in gaming, have led to the formulation of many abstract mathematical assessment models. The result is that many games require a large volume of input data; in many cases, the required constants, and indeed even the mathematical functions, are simply not known or available. This lack of validated data generally results in the use of estimated functions and constants. Although estimates are made so as to take advantage of any experience that may exist, this factor adds considerably to the uncertainties associated with gaming results. Often proponents of computer simulations, as opposed to multiperson gaming, fail to recognize that this constraint is really common to both types of simulation. But it is, and its severity can often be partly alleviated through carefully-controlled field experimentation or through the application of experimental-design techniques to game play.

On the other hand gaming is often necessary to determine the significant data that ought to be collected in the field. This is one of the objectives that can be achieved within, or perhaps in spite of, the limitations already discussed.

Predicting Future Real-Life Outcomes

Notably absent from the list above (pages 188-189) of objectives achievable through gaming is that of predicting future real-life outcomes. This objective is absent not because gaming has no utility in predictive processes but because statements about its utility require considerable qualification. In the area of typical war games this has been a highly controversial point, with some gaming enthusiasts insisting that a high

degree of prediction is possible, those not readily accepting gaming as a
useful procedure insisting that no prediction is possible, and the range in
between these extremes adequately covered by intermediate opinions.
Much of this controversy has been clouded in differences in semantics and
imprecision in definition of the type of outcome to be predicted. But what
of the substance of the issue? Can reliable and valid predictions be made
through the gaming technique?

The question of making predictions is very complex and one that can-
not be treated adequately within the scope of this study. An unqualified
answer to the question and one not applicable to a particular game, set of
players, and problem is almost certain to be wrong. Individual games
differ too greatly to permit easy generalization. But reliability is a rela-
tive term, as is validity; consequently, unless one establishes a basis for
comparison, the question posed is not too meaningful except in the narrow-
est of contexts.

One basis of comparison that is often, and perhaps erroneously, cited
is that of the ability to predict physical experimental science (see Hendrick-
son and Girard, in 33). Since experiments that are typical of the physical
sciences are usually replicable and have input parameters that are almost
completely controllable, one can establish a theory, test it adequately, gain
its acceptance by the scientific community, and place great confidence in
its predictions. This is perhaps true to a lesser extent in social sciences
where the parameters are not as controllable and the experiments not as
replicable—usually because of the human variable so evident in gaming.
Thus in general it would appear that the predictive value of gaming must
necessarily be rated low relative to that of rigidly controlled experimental
techniques used in the physical sciences, and in view of the present state
of the art, logic would suggest that it be rated somewhat low with respect
to the same yardstick for experimentation in some of the social sciences.
But experimentation in most aspects of military effectiveness, particularly
in the combat phases, simply cannot be performed in the rigidly controlla-
ble manner in which physical experiments can; moreover, since physical
science experimentation generally treats problems of much more restricted
scope and complexity than does the typical war game, it does not in fact
provide a good basis for comparison. The explosiveness of a chemical is
a military problem and can be investigated quite rigidly. Its military ef-
fectiveness is another matter.

If a realistic basis for comparison is to be obtained, the validity and
reliability of the predictions associated with the results of each problem
whose solution is attempted through gaming should be compared with those
associated with the results obtainable through other available and practi-
cable techniques. As far as is known there are no data that permit this
type of comparison for games in general, nor any that compare real-life
results with game predictions. Such data would be extremely difficult to
secure, especially for most modern military games; these often treat con-
tingencies that have not occurred, and if they do occur, they will probably
not be precisely as supposed in the play of the game. A thorough exami-
nation of the question posed is a formidable task, and one that must await
further research.

Nevertheless it is generally conceded that there are limitations on the
reliability and validity of predictions based on individual game outcomes

and that these limitations apply with greater force as the outcome under consideration becomes more specific (35). Consequently greater confidence is attached to predictions based on several independent plays preferably with different game models. For individual outcomes of individual games with given sets of players, each prediction must be examined on its own merits, and the appropriate measures of reliability, validity, or confidence to be attached to them are at present very much a matter of subjective judgment.

Such judgment enters also as one attempts to understand better why the research objectives previously listed (pages 188-189) appear to be achievable through gaming.

Multiperson Gaming as an Aid in the
Development of Computer Simulations

Since computer simulations are generally considered to be more objective and since the parameters are more controllable and therefore better opportunities for obtaining predictive results are afforded, it is often desired to produce one-person games or computer simulations of complex systems, such as, say, the continental air defense system. In attempts to simulate such systems, it often proves desirable to engage in many different types of analyses and at some stage to incorporate the results obtained in a game. The process that this implies may be illustrated, though not at all in detail, by considering a possible analysis of the continental air defense system.

If one desired to study requirements for interceptor forces, local air defense units, and anti-intercontinental ballistic missile forces in the defense of the United States, it is conceivable that several combinations of these systems (or of alternative ones), several possible deployments, several levels of total effort, etc., might need to be examined for a variety of attack patterns. It would be desirable ultimately to prepare a computer simulation that resembled the OMEGA air-battle model (33) in many respects, including (a) models simulating the performance of surface-to-air missile systems against bombers, submarine missiles, and inter-continental ballistic missiles, and (b) models similarly simulating the performance of interceptors. In the first state of model development, it would be difficult enough to simulate the performance of isolated local defenses of various types and that of single interceptor squadrons and to prepare models for computer assessment of their performance. Once this task were accomplished it would be possible to assess the effects of attacks on various systems and deployments, perhaps through iterative use of the same basic models. But at this stage a new level of complexity enters, for one must seek to define and explore realistic attack patterns and realistic defense system responses; the initial exploration of the system aspects could well include a gaming test or exercise.

The purpose of a game at this stage of research might be confined to the exploration of the interaction of the various defense systems and of the roles that men and organizations play as they use these systems and develop attack strategies and defense allocations. The game would thus provide a vehicle for incorporating into the analysis the experience, creativity, and powers of integration possessed by many minds. The results would not necessarily prove anything about the systems studied ("prove" in

the sense that the results of experiments prove a theory), but they might
suggest general principles and approaches that would permit the computer-
model builders to incorporate many system aspects into what was formerly
a sequence of isolated defenses and to begin the development of a national
defense model. Valuable insight might come out of such plays, and one
might even learn that additional and different weapon systems or modifica-
tions of the ones under study were needed; or the game could assist in de-
fining probable tactics, strategies, and principles of deployment and opera-
tion to the point that they would be identified in sufficient detail to permit
the establishment of a computer simulation in which the only decisions to
be made were dollar allocations to various defense systems.

This example illustrates a typical cycle of model development in
which one prepares an abstract simplified quantitative model of a complex
situation as a building block in the simulation of a much larger system;
this is followed by a gaming cycle in order to lend credence to the abstrac-
tion already made and to obtain the insight necessary for the next level of
abstraction; this in turn is followed by new attempts at analytical abstrac-
tion, new gaming efforts, etc. Gaming has been used quite successfully in
this type of analysis at the Research Analysis Corporation and RAND.*

The use of gaming illustrated here is only one example of the way in
which gaming is used in conjunction with other and more traditional types
of research to analyze problems of great scope and complexity. Many of
these uses grow out of the ability of games employing suitably constructed
computer-simulation models to provide data of the types previously
indicated. But gaming has become a tool for assisting in evaluating the
capabilities of very large forces and of testing the feasibility of innova-
tions that are arrived at through various processes. These uses are best
illustrated by examining a typical research and development planning
cycle, one that is applicable at many levels throughout the military
establishment.

Gaming as a Part of the Research, Development,
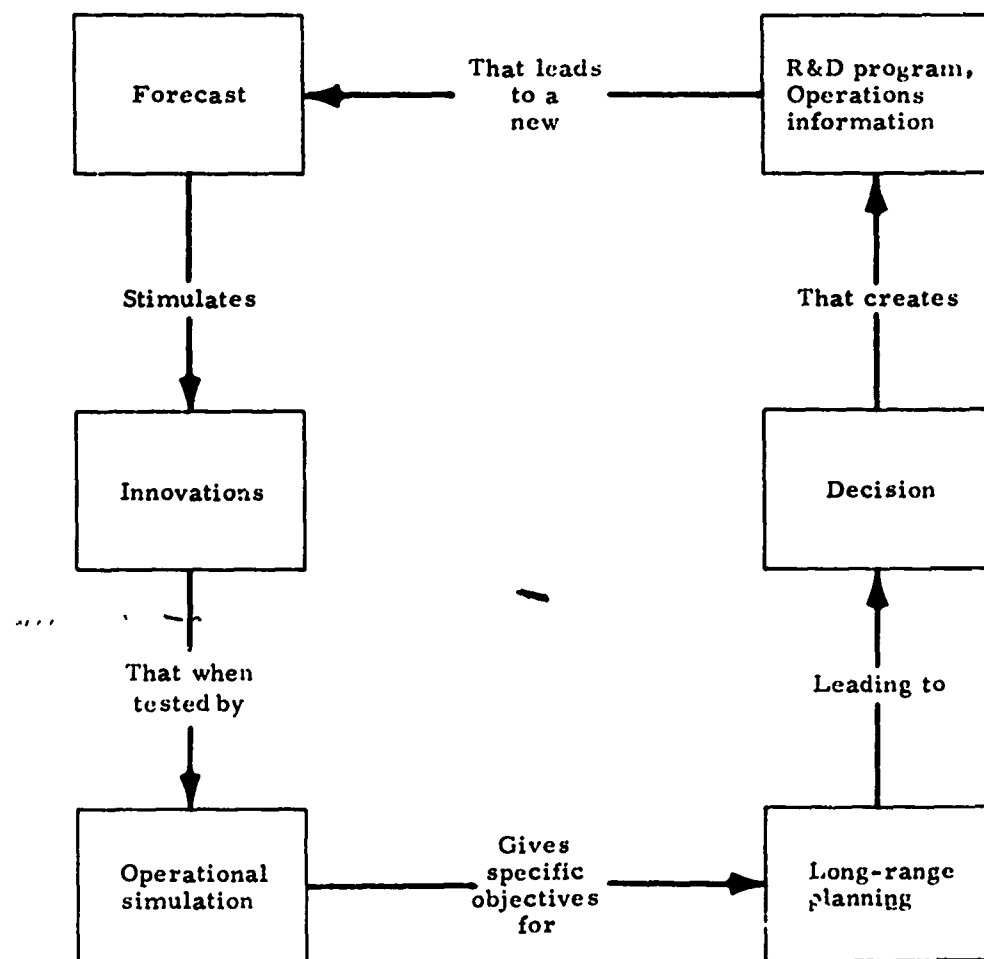and Planning Cycle

In discussing the more extended role of gaming as a part of the re-
search and development cycle that pervades all echelons of the military
establishment, a figure that was used frequently at the Operations Re-
search Office and in particular by Dr. E. A. Johnson in his keynote speech
to the Third Symposium on War Gaming at the University of Michigan is
used.

The figure which follows illustrates a sequence of actions that take
place periodically (and somewhat simultaneously) throughout various parts

---

*See Enke (15) for another example in which gaming has been used as
a part of a much larger research effort. He states in the abstract to this
document that it is "an attempt . . . to present a case study in the use of
laboratory simulation as a complement to more traditional economic re-
search into the efficiency of large organizations. While human simulation
plays a part in comparing, testing, and evolving policy innovations . . .
gaming of this kind must be an adjunct of a larger research effort that uses
traditional means of research."

of the several services, within the Joint Chiefs of Staff, and also in the National Security Council. At some time these organizations undertake a forecast of the probable future in which many contingencies are considered and the possible outcomes of military action evaluated. The results of this forecast stimulate innovations in strategy, tactics, weapon systems, and sometimes individual weapons. These new ideas are then given a feasibility test through some form of operation simulation, the results of which alter previous objectives for long-range planning. The changes in long-range-planning objectives give rise to decisions that create research and development programs and new information for planning operations in various contingencies. These are considered at the next evaluation and in turn give rise to a new forecast that causes the cycle to be repeated. This is, of course, an oversimplification of a very complicated process, but it is reasonably understandable as an abstraction.

Sequence of Actions During R&D Planning Cycle

Forecast	That leads to a new	R&D program, Operations information
Stimulates		**That creates**
Innovations		Decision
That when tested by		Leading to
Operational simulation	Gives specific objectives for	Long-range planning

Gaming enters this cycle quite directly, often as a basic technique in making the evaluations needed for the forecast and as a form of operational simulation in the feasibility tests. The results of the feasibility tests will form the basis for acceptance or rejection of the new idea and also for the revision of long-range plans. Finally gaming is used in facilitating the implementation of the research and development programs generated by the new plans and in providing data necessary to prepare operational plans that are to be evaluated in the next forecast.* It is not the only technique used for these purposes, and its use is subject to the limitations previously discussed, but in the absence of better techniques its use in these phases has become widespread.

ROLE OF SOCIAL SCIENCE IN FUTURE GAMING

On several occasions the role that various social science skills and information have come to play in gaming has been indicated. As one looks to the future, this role seems to be an inevitably increasing one. The need for historians to assist in abstracting from the past those lessons that may reasonably be applied even in somewhat modified form to the future; for experimental psychologists to assist in experimental de 'gn and in the conduct of play; for political scientists and sociologists in injecting into military war games those realistic political factors that may constrain the military action or alter its course significantly; and for sociologists, psychologists 'd anthropologists to assist in including in gaming those aspects of culture (economic, geographic, etc.) that tend to determine human behavior both in the real world and in the simulated situation have all been suggested. This is clearly a very limited iteration of skill and information requirements, and thus far the future of such activities has been considered in even briefer form. But, as indicated and illustrated through brief consideration of the implications of nuclear weapons in a European war, because gaming is being used to assist in the study of increasingly more complex military problems and because these problems involve individual and group behavior and many other related social and cultural phenomena, one must expect that gaming will find greater use for social science.

There are at least three areas that are closely associated with military functions in which this appears to be certain to be true: the cold war,** civil defense, and arms control.*** All are receiving progressively greater attention at the present time, all are extremely complex, and in all three areas behavioral, economic, cultural, social, and political factors are extremely significant. It would appear that the mere complexity of these problems and the lack of adequate analytical techniques to study them in all their ramifications will lead analysts to turn for assistance to gaming in some form and also to social science. Moreover, as social scientists seek to probe these and other complex areas in greater depth, they will probably find gaming a progressively more useful tool, as have the operations analysts.

---

*See 13, 18, 33, and 37 for examples of games that are used as parts of the research and development cycle.

**See Goldhamer (19) and Price (36) for illustrations of coldwar gaming.

***See Katz (26) for an arms-control problem in which gaming could be used advantageously.

REFERENCES

1. Archer, W. L. "The Technique of Modern War Gaming." Canadian Army Journal, 15 (1961), 15-25.

2. Armament Research and Development Establishment. "On the Use of a Digital Computer for Playing War Games." Great Britain, ARDE Rept. (B) 14/59 (August 1959).

3. Army Electronic Proving Ground. "Development of a Model to Simulate Military Communication Systems," Ft. Huachuca, Ariz., Department of Army, Signal Corps, Electronic Proving Ground, A :PG-SIG-940-39.

4. _____. "Instruction Manual for the IBM 704-709 Computer," Ft. Huachuca, Ariz., Department of Army, Signal Corps, Electronic Proving Ground, AEPG-SIG-940-46 (January 1959).

5. _____. "War Gaming for Signal Systems," Ft. Huachuca, Ariz., Department of Army, Signal Corps, Electronic Proving Ground, AEPG-SIG-940-40 (October 1958).

6. Beale, E. M. L. "The Role of Gaming in Military Operations Research." Great Britain, Admiralty, DOR Memo No. 190 (January 1961).

7. Benson, Oliver. "A Simple Diplomatic Game," American Political Science Association, September 1959. (Mimeo.)

8. Beverly, R. S. Communications Analysis in LP-I. (RAND paper no. P-1457.) Santa Monica, Calif.: RAND Corporation, 1958.

9. Bloomfield, Lincoln P. "The Political Exercise--II. U.S. and U.S.S.R. in Iran: (The Plan of CENIS Sponsored Game Based on Hypothetical Civil-War Situation in Iran in U.S.S.R. Supported Opposite Side)." Cambridge, Mass.: Massachusetts Institute of Technology, Center for International Studies, December 1960.

10. _____,.and Padelford, Norman J. "Three Experiments in Political Gaming," American Political Science Review, 53 (1959), 1105-1115.

11. Davison, W. P. A Public Opinion Game. (RAND paper no. P-2042.) Santa Monica, Calif.: RAND Corporation, 1960.

12. Davy, L. N. "A Mathematical Model of Infantry Combat for SYNTAC Use." Ft. Monroe, Va., Technical Operations, Inc., USCONARC Headquarters, CORC-M-98 (May 1960).

13. Doughty, Joseph M. "A Simulation for the Experimental Study of Decision Making in Complex Military Systems." Bedford, Mass., Department of Air Force, Command and Control Division, Tech. Note 60-32.

REFERENCES (Continued)

14. Ellis, J. W., and Greene, T. E. "Contextual Study: A Structured Approach to the Study of Political and Military Aspects of Limited War," *Operations Research* 8 (1960), 639-651.

15. Enke, Stephen. *On the Economic Management of Large Organizations: A Case Study in Military Logistics Involving Laboratory Simulation.* (RAND paper no. P-1368.) Santa Monica, Calif.: RAND Corporation, 1958.

16. Gainen, L., et al. *Baselogs--A Base Logistics Management Game.* (RAND memorandum no. RM-2086.) Santa Monica, Calif.: RAND Corporation, 1958.

17. Geisler, Murray A. *Man-Machine Simulation Progress.* (RAND paper no. P-2086.) Santa Monica, Calif.: RAND Corporation, 1960.

18. General Dynamics Corporation. "Gaming: A Method of Studying Submarine Tactics." San Diego, Calif., General Dynamics Corporation, CONVAIR Division, P.O. Box 1950 (August 1958).

19. Goldhamer, Herbert. *Toward a Cold War Game.* Santa Monica, Calif.: RAND Corporation, 1954. (Unpublished.)

20. _____, and Speier, Hans. "Some Observations on Political Gaming," *World Politics*, 12 (1959), 71-83. (Also RAND paper no. P-1679-RC; Santa Monica, Calif.: RAND Corporation, 1959.)

21. Greene, Jay R., and Sisson, Roger. L. *Dynamic Management Decision Games.* New York: John Wiley & Sons, 1959.

22. Guetzkow, H. "A Use of Simulation in the Study of Inter-Nation Relationships," *American Behavioral Scientist*, 4 (1959), 183-191.

23. Helmer, Olaf. *Strategic Gaming.* (RAND paper no. P-1902.) Santa Monica, Calif.: RAND Corporation, 1960.

24. Hodgson, James, "A Generalized Analysis of War Gaming." Ft. Monroe, Va., Technical Operations, Inc., USCONARC Headquarters, CORG-M-66 (April 1957).

25. Johnson, Robert E., and Wilson, William H. "Performance of Certain POL Transport Systems under Conditions of Nuclear Warfare." Ft. Monroe, Va., Technical Operations, Inc., USCONARC Headquarters, CORG-M-77 (January 1959).

26. Katz, Amrom H. "Hiders and Finders," *Bulletin of the Atomic Scientists*, 17 (1961), 423-425.

27. Kingsley, Edward H. "OCAMA Version of the Spare Part Manager's Game: Monopologs." Oklahoma City, Okla., Department of Air Force, Operations Analysis, Oklahoma City Air Material Area, Working Paper No. 2 (November 1958).

REFERENCES (Continued)

28. Leutert, W. W. "Simulation of a Battle on High Speed Digital Computers." Department of Army, Ordnance Corps, Aberdeen Proving Ground, Ballistic Research Laboratories Rept. No. 911 (June 1954).

29. Michael, Donald N. "The Social Environment," Operations Research, 7 (1959), 506-523.

30. Mood, Alexander M., and Specht, R. D. Gaming as a Technique of Analysis. (RAND paper no. P-579.) Santa Monica, Calif.: RAND Corporation, 1954.

31. O'Neill, R. R. "Analysis of Cargo Handling--Part V, Simulation of Cargo Handling Systems." Los Angeles, Calif., University of California, Contr Nonr 233(07), September 1956.

32. Orcutt, Guy H. "Simulation of Economic Systems," The American Economic Review, 50 (1960), 893-907.

33. Overholt, John (ed.). "First War Gaming Symposium Proceedings." Washington, D.C.: Washington Operations Research Council, November 1961.

34. Padelford, Norman J. "Report on Political Exercise on Berlin." Cambridge, Mass.: Massachusetts Institute of Technology, Center for International Studies, January 1959. (Mimeo.)

35. Pearman, J. P. T., and Whitmore, W. F. "Limitations on the Predictive Use of War-Gaming Techniques." Paper presented to the Fifth National Meeting of the Operations Research Society, Washington, D.C., November 10-20, 1954. (Abstract in Journal of the Operations Research Society of America, 3 (1955), 121.)

36. Price, Maurice T. "War Gaming the Cold War." U. S. Naval Institute Proceedings, 85 (1959), 44-47.

37. Riley, Vera, and Young, John P. "Bibliography on War Gaming," Bethesda, Md., Operations Research Office, Johns Hopkins University (now Research Analysis Corporation), April 1957.

38. Smith, Nicholas M., and Marney, Milton. "Simulation and Gaming as Cognitive Models," Bethesda, Md., Operations Research Office, Johns Hopkins University (now Research Analysis Corporation), January 24, 1961. (Multilithed.)

39. Technical Operations, Inc. "Joint War Gaming Feasibility Study," prepared for Defense Atomic Support Agency (ORO, APL, WSEG, and Laboratories for Applied Sciences of the University of Chicago participated in the study), April 28, 1961.

40. Thomason, Joei F. "Testing High-Level Plans with War Games," Carlisle, Pa., Department of Army, U.S. Army War College (Student Individual Study), March 15, 1955.

REFERENCES (Continued)

41. Thompson, F. B., et al. "War Gaming Rules," Santa Monica, Calif., General Analysis Corporation, 1710 Wilshire Blvd., under contract no. DA-36-039-67843, Department of Army, Signal Corps, Electronic Proving Ground, April 10, 1957.

42. Thornby, John T. "The Use of Computers in Decision Making," Minneapolis, Minn., Experimental Design and Mathematical Research, The Pillsbury Company. (Abstract in Operations Research, 8, Suppl. 2 (Fall 1960), 109-110.

43. Weiner, M. G. War Gaming Methodology. (RAND memorandum no. RM-2413.) Santa Monica, Calif.: RAND Corporation, 1959.

IX

ARMS CONTROL

Thomas C. Schelling*

The traditional role of disarmament has been one of simple opposition to national armament. Disarmament has meant the reduction of military force. And while the achievement of disarmament could be a matter of degree, the popular philosophy of disarmament was not easily reconciled with dependence on national military establishments. Security could be organized either on the basis of military defense, military deterrence, and military equilibrium of some sort, or on the basis of law and order, trust and good will, mutual respect, peaceful resolution of conflict, and a universal denial of international violence. Additionally, the "disarmed world" has often been viewed as one in which nations are not armed against each other but are subordinate to some universal military authority that polices the world against violence, respects the integrity of nations, and is itself immune to abuse, internal revolt, or the temptations that go with a monopoly of power.

Proponents of disarmament have traditionally assumed that if military force is available it will tend to be used, that military establishments "militarize" international relations, and hence that the greater the military force available the more likely is war and the worse the war will be. While disarmament proposals have sometimes been selective and not wholly indiscriminate, the predominant emphasis has been <u>downward</u> in direction, whether in budgets, manpower, or firepower.

CONCEPTS OF ARMS CONTROL

In recent years, principally in the United States, some different conceptions of arms limitation have arisen. They have, with some ambiguity

---

Thomas C. Schelling, PhD, is Professor of Economics at Harvard University and a Faculty Associate of Harvard's Center for International Affairs. He divides his time between the theory of conflict and the military aspects of foreign policy. His interest in both these subjects was stimulated by five years' work with the foreign aid programs in Copenhagen, Paris, and Washington. Dr. Schelling has been a regular lecturer at several of the war colleges, is a member of the Scientific Advisory Board of the Air Force, and has been a consultant to the Departments of Defense and State and to the Arms Control and Disarmament Agency.

---

and some discomfort, acquired the name of "arms control." However clumsy these words are in identifying the difference, there is a difference that is noticeable in the motives, attitudes, and even in the people involved, as well as in the programs advocated.

The difference in the people involved is conspicuous. Those who in recent years have concerned themselves with arms control are in no sense people who renounce an interest in military security, distrust military establishments, and advocate a simple reduction in arms levels. As a matter of fact, the intellectual leadership has come substantially from people who identify themselves with national security policy and are professionally concerned with military affairs.

Those who are presently interested in arms control are, for the most part, unwilling to see the policy choice as one between two extremes: blind reliance on military force and complete renunciation of it. They tend to view military security as a technically complex matter, and they distinguish between the kinds of weapon systems and military capabilities that enhance the likelihood of war and those that reduce it. They are as much concerned with military incentives as with military capabilities, but they see the incentives not simply as gross temptations to use available military force or as the consequence of "military thinking" in foreign policy.

Instead, those who are interested in arms control see the incentives as structured by the relation of military forces to the decision process. They are concerned with vulnerability to surprise attack, with the military advantages in the pre-emptive initiation of general war; with the problems of false alarms, hasty decision, "accidental war," and limitation or escalation of limited wars; and with the level of violence if war occurs. They are concerned about the chain of command, military communications and warning systems, and the delegation of military authority. They are concerned about local imbalances of forces, and they may worry as much about military vacua in particular localities as about military confrontations elsewhere.

The arms control advocates recognize that a balance of terror is to be measured not only by the latent destruction it can unleash but also by the likelihood that it may be upset; and this in turn is seen to depend on factors more complex than just the speeds of delivery and the total megatonnage in inventory.

Surprise and Vulnerability

The surprise-attack negotiation between East and West in late 1958 was influential in the development of an arms-control approach. In 1958, the United States and the Soviet Union became committed to "technical" discussions of the surprise-attack problem. With some urgency American officials began to inquire more deeply than before just what was peculiar to "surprise attack" as an arms-control problem. The negotiations themselves stimulated intense thought about surprise attack among a number of people professionally concerned with military policy, and a consensus slowly emerged that was hard to square with the traditional approach to disarmament.

Central to this consensus was the realization that the acquisition of substantial strategic-nuclear strike forces by both sides did not automatically result in a stable balance of mutual deterrence. The surprise-attack problem between major powers was seen to be a function of the military advantage of striking <u>first</u> in a general war. This advantage lay in the possibility that the attacker could destroy a substantial part of the force whose existence was supposed to deter his attack. The more vulnerable the forces on either side to sudden attack, the greater would be the advantage to the side that started the war. The vulnerability of strategic retaliatory forces was not only a temptation to initiate general war, but a source of anxiety that might lead, particularly in a crisis, to an initiation of war in "anticipatory self-defense."

By early 1959, something of a revolution in strategic thinking had occurred; "vulnerability" was its focus. It became widely appreciated that the true measure of the deterrent potency of a strategic nuclear force was not the total megatonnage it could muster for peacetime parade but the threat of the damage it could still do with those of its forces that could survive an attack. The surprise-attack negotiations in 1958 undoubtedly accelerated the appreciation of the vulnerability problem. It is significant that Albert Wohlstetter, the author of the piece of literature (46) that had the greatest impact on thinking about vulnerability, was on the United States staff at the surprise-attack conference. That"disarmament" conference accelerated the reception of Wohlstetter's point of view. Policymakers and professional strategists became aware, and began to articulate, that the <u>stability</u> of the balance of deterrence, and not merely the nuclear weights in the balance, was of prime strategic importance.

The anomaly was substantial. Out of a "disarmament" conference came a focus on the need to protect strategic retaliatory forces—not women and children, cities and industrial assets. From this conference also came an interest in a system of more stabilized deterrence, in which both sides' retaliatory forces are secure enough to reduce substantially the danger of deliberate attack or preemptive attack in a crisis.

Once "disarmament" becomes compatible with the "improvement" of our strategic forces, the traditional antithesis between disarmament and military force becomes confounded. Not only does the protection of the Strategic Air Command become consistent with some kinds of "disarmament," but also attention is drawn to the fact that many improvements in a military establishment—better communications, more reliable command and control, warning systems with less proclivity to false alarm, etc.--are consistent with the spirit of disarmament. Even potential adversaries should value these kinds of <u>improvements</u> in our military forces, just as we should feel more secure if potential adversaries improve their own strategic forces in these directions (see Schelling, 40).

But the word "disarmament" can hardly be literally applied to <u>improvements</u> in military forces. Of course, in a semantic sense, one could say that taking a firing pin out of a weapon "disarms" it and that the development of better and more expensive weapons that are less sensitively "armed" is a kind of disarmament. One could also say that to protect our strategic forces is to degrade an enemy's capability to attack them; so we mutually "disarm" each other when we spend money to reduce vulnerability of our strategic forces. Thus, some literal meaning of "disarmament"

could be salvaged, but clearly we are not dealing with disarmament as traditionally intended.

Furthermore, there is more to the "disarmament" point of view than a simple bias toward lesser armament and lesser expenditure on it. It can be illustrated by the probable attitudes toward a program of very drastic disarmament. The "arms controller" would examine the military implications of very substantial disarmament in order to determine the effect of this arms level on the stability of the strategic balance. He may ask whether such reduced numbers of strategic weapons on both sides aggravate or reduce the advantage of pre-emptive attack and whether they aggravate or reduce the advantage that would accrue from rapid or clandestine rearmament. Even as an "arms controller" he might conclude that drastic disarmament, if adequately monitored, could bring an improvement in security--on condition that it has been carefully designed with respect to residual and latent military capabilities and mobilization potentials, if these latent capabilities are structured to preserve some kind of potential retaliatory power, and if this is done in a way that keeps attention focused on a need for military deterrence.

Those who advocate "disarmament" but distrust "arms control" would probably say that this drastic reduction in armament would lead to a peaceful world only under contrary conditions--if we do not insist on preserving our military preoccupations, intensifying our interest in military balance, thinking in military terms, and continually assessing the relative merits of initiating war or absorbing the first blow.

Thus the main difference between the "arms controllers" and the "disarmers" is probably not in the level of armaments they would settle for, but in the way they would analyze the military and political implications of a low level of armaments. In this sense the arms controllers can be characterized as those who hope to improve military policy through understandings reached with our potential enemies, while the disarmers are those who hope to eschew military policy through understandings reached with our potential enemies.

So much for the contrast between traditional disarmament and what is now called arms control. What about the relation of arms control to traditional military strategy?

Military Strategy as Bargaining Strategy

Perhaps the best way to characterize arms control is to say that it represents an increased emphasis on the collaborative aspects of our military relations with potential enemies. It is a recognition that modern military strategy--perhaps most military strategy throughout the ages, but particularly modern military strategy--is essentially a strategy of international bargaining involving the potential use of military force. "Potential" is important here. Modern military strategy is not wholly concerned, probably not mainly concerned, with the efficient destruction of an enemy. It is much more concerned with influencing potential enemies--with affecting enemy intentions as well as enemy capabilities. (The bargaining is, for the most part, tacit rather than explicit--it is bargaining by actions and responses as much as by words, implicit as well as explicit threats, understandings rather than enforceable agreements.)

The concept of deterrence is that we can induce a potential enemy, when he looks at the options available to him, to consider how we might respond to each of these options and, taking these probable responses into account, choose a course of action that we would want him to choose, one that is not intolerably aggressive or provocative. The concept of limited war is also a bargaining concept; it is the latent threat of withheld military potency that provides the inducement to keep a small war limited. Thus modern military force is essentially, though not wholly, a source of bargaining power in international diplomacy, especially in our diplomatic relations with our main potential adversaries.

We tend to think of military force in conjunction with threats. But promises or reassurances are just as much involved. We try to deter enemy attack by threatening, "If you transgress, we shall attack." But the threat works only if there is also an implicit promise, "And if you don't, we won't." To say that in limited war we threaten to enlarge the war if the enemy does not limit his actions and objectives is simply to say that we offer to limit our response if the enemy limits his actions. Or if we say that we shall boost our military budget by twenty billion dollars a year if an enemy engages in anything as provocative as Korea, we are in effect offering to keep our military budget lower than it might otherwise be if the enemy restrains his provocations. An effective threat is a conditional commitment to act, and, conversely, a threat is also an offer to abstain from certain actions if the enemy, too, will abstain.

Of course, what we threaten would often be as painful for ourselves as for the enemy. To punish the enemy by initiating war would be the greatest disaster that this country has ever experienced. But to say this does not contradict the notion that the obverse of a threat is a reassurance; it only emphasizes the fact that we have a substantial common interest with our enemy in the avoidance of mutually disastrous military outcomes and activities. It also makes clear that pursuit of a skillful military policy will require that we gear our expected reactions to enemy actions in such a way that his perception of his own best interest coincides substantially with ours.

But if this general view of military policy is accepted, it becomes difficult to disentangle the collaborative or "arms-control" aspect from the rest of our policy. Analytically we can distinguish the conflict between us and our adversaries from the common interest we have with them. Verbally we can try to distinguish between a threat to retaliate if attacked and a promise to abstain if not attacked. Analytically we can distinguish between the first-strike potency of our strategic nuclear forces and their second-strike potency and can recognize that our opponent may well deprecate our development of a first-strike capability while possibly approving our development of a more secure second-strike capability. But when we come to incorporating these two elements, the antagonistic and the collaborative, in actual programs, they are difficult or impossible to disentangle.

Furthermore, there is no reason to associate "military" policy with a desire for war, with pure antagonism to all possible potential enemies, and with an urge toward military destruction. Military policy has the purpose of furthering national objectives, especially national security. If military policy can deter aggression or compel withdrawal, it has served its purpose. The destruction of enemy targets at great cost and risk to ourselves

is not the ultimate objective of military policy. Hence, : ere is no need to identify military policy with the purely antagonistic and destructive aspect of military potential.

A well conceived military policy should therefore be as concerned to induce a potential enemy to remain quiescent as to oppose or destroy him when he transgresses. Furthermore, wise military policy should recognize that a sharp increase in military preparedness imposes on the enemy an incentive to increase his own preparations. Restraint in the arms race is not anti-military if it induces potential enemies to abstain from increasing their own military buildup. Just as we can match enemy capabilities by raising our expenditures on military force, we may possibly induce the enemy to spend less if we spend less. Military policy should be concerred with the response of potential enemies to the size and quality of military force that we create.

In that sense, military policy can be just as "diplomatic"--as responsive to the performance of other countries and as much a matter of bargaining and negotiation--as economic policy or any other kind of diplomacy, but only if military policy is skillfully conducted.

If responsive, it already in principle incorporates arms control within it. Military policy is aimed at maximizing our security; and if it is well informed and well rounded and takes the responses of potential enemies into account, it will lead to large or small preparations, first- or second-strike capability, nuclear or conventional capability, or aggressive or deterrent postures, depending on the response or expected response of potential enemies. We may estimate that we can get more security at lower levels of armaments, or with more second-strike capability at the expense of first-strike capability, or with better command and control and less megatonnage, because enemy postures in some fashion and in some degree reciprocate ours. Then a wise military policy is one that takes this enemy response into account and that seeks maximum security, not maximum military potency.

I could conclude at this point that arms control is simply a dimension of military strategy and not a special category of analysis or policy. If so, I should have to add that it is a dimension that has been underemphasized in the past. Arms control as a branch of policy could then be viewed as a belated interest in, and emphasis on, this particular aspect of our military diplomacy. Logically, this is the only conclusion to reach. And if arms-control thinking becomes sufficiently pervasive in the military establishment, then we might look forward to the withering away of arms control as a special branch of policy and its eventual incorporation in a more comprehensive, and better integrated military strategy.

Furthermore, arms control is not exclusively, perhaps not mainly, a matter of negotiation and administration nor of formal international arrangements. Not all of our relations with friendly countries are embodied in treaties and formal organizations; neither are our military relations with potential enemies. Mutual restraints may be formally negotiated, embodied in treaties, and monitored by formal organizations; but they need not be. In fact, if arms control is defined as I have tried to define it, as a recognition of the collaborative element in our military relations with potential enemies, these relations are quickly seen to be informal as well

as formal, implicit as well as institutionalized. There is no formal treaty explicitly oriented toward the non-assassination of each other's heads of state, scientists, or military leaders, but the major powers appear to abstain from it. There is no formal treaty that we shall not double our defense budget unless our adversaries adopt particularly obnoxious policies, and yet we hold that doubling in abeyance. We have never negotiated an understanding that our airborne alert would remain modest unless world conditions deteriorate; whether we call this a threat of enlarged airborne alert, or a promise that airborne alert will not be magnified unless conditions require it, there is informal reciprocity here.

Each side perceives that the other's behavior is contingent on its own. While understandings may be clarified by formal negotiations and reinforced by negotiated agreements, it is not the formal document that constitutes the arms control. A negotiated treaty can be part of the framework, but it is not the substance of control. The substance is mutual abstention, collaboration, and reciprocated self-control. The institutional mechanisms by which the understandings are reached, and in which they are embodied, important as they may be, are not the essence of arms control. The essence is in the actions, expectations and understandings of countries which militarily react to each other and which appreciate that they do.*

RESEARCH IMPLICATIONS ·

The following sections of this study attempt to sample the myriad problems and opportunities of arms control and to identify those that may be better understood by the application of professional social-science research. I want to emphasize here that I have not tried to be comprehensive. I have ignored some areas in which research is of obvious importance,** and I have abstained from proposing that we need to increase our knowledge of military strategy, of international relations, of political decision-making, and of everything in general. I have tried to focus on some concrete areas in which systematic research may appreciably improve our policy capabilities in the immediate future. I have tried to pick a variegated selection for its illustrative and suggestive value rather than to pick the priority items on the menu of possible research.

---

*For a fuller statement of the conception of arms control presented above, and an exploration of some of the problems and opportunities, see Schelling and Halperin (41). See als , 'll (7); the collected essays in Brennan, ed., (6); Lefever, ed., (27),   nkin, ed., (19), and Schelling (40,.

**A sample of important problem left out: historical studies of particular arms races, of arms-control i gotiations, and of truces, disengagements, and arms limitations; political and military problems of a world organization or a major-power undertaking to police the world against war and rearmament; studies of particular arms-control proposals--man-power limitations, strategic-vehicle limitations, nuclear-weapon bans, disengagements and nuclear-free zones, space prohibitions, launch registration and advance notification; arms control and the small countries; techniques of inspection and surveillance; techniques of authentic communication; the nature of the cold war in a disarmed world; weapon definitions and classifications; the economic implications of alternative arms budgets.

Research will get done mainly because there are qualified people who can be interested in it. Financial support can help, but moral support, suggestion, and the assurance of a receptive audience are as important as funds. Identifying fruitful lines of inquiry, particularly those that have intellectual fascination, is at least as important as developing a "program." In order to be suggestive, I have tried to select a variegated list of topics; even so, my own narrow interests are undoubtedly reflected in the discussion that follows.

Study of Existing Arms Controls

If the definition of arms control is broadened to include restraints arrived at without formal negotiation and agreement, it becomes evident that we and the communist bloc already participate in a great deal of arms control. We abstain from many kinds of hostile acts, international mischief, and arms competition that might be advantageous if unilaterally engaged in Such acts would so likely induce reciprocation and thereby create new risks and costs for both sides that they are not considered expedient. As mentioned, we do not engage in assassination. For the most part we do not spoof each other's warning systems, jam or otherwise interfere with each other's military communications, fire practice rounds at each other's territories, or engage in much, if any, peacetime sabotage.

Both sides have also displayed a reluctance to spread nuclear weapons among their allies. This restraint must stem in part from an estimate that the open distribution of nuclear weapons to its allies by one side will put the other side under strong pressure to reciprocate. Soviet provision of nuclear weapons to China or to Egypt would undoubtedly reduce our inhibitions against sharing nuclear weapons with our allies, and allied demands might become almost irresistible.

It seems likely that certain dimensions of the arms race have been damped by a conscious or semi-conscious appreciation that what one side could do the other could do as well. Quite possibly, one of the many reasons for our remarkable abstention from civil defense programs for several years was a hope not to engage in a civil-defense competition with the Soviets. Partly also, it may have been due to a general reluctance to engage in excessively warlike programs for fear that the process of reciprocation would contribute, by feedback, to a more dangerous arms race. There is some evidence, moreover, that arms-budget decisions have been somewhat constrained by the recognition that a sudden increase in expenditures on both sides might only result in a worsening of the situation all around. The reluctance of the United States to embark on an airborne alert in early 1959 may have been based partly on a desire to avoid provoking increased Soviet efforts in other areas of military activities.

It should be recognized that there is a certain symmetry between threats to enlarge the arms race and offers to keep it under control. To the extent that we have a reserve capability for substantially increasing the arms budget, and that we make clear our intention to do so if an adversary sufficiently provokes us but not to do so otherwise, then we are abstaining from an increase in the arms budget on condition that the other side abstain from certain actions. To recognize that we have latent responses that have not yet been provoked is to recognize that certain

conditional mutual restraints may exist.* The announced budget increases
related to the Berlin crisis in mid-1961 illustrate (in the upward direction)
the possible explicit interaction of budgetary decisions here and in the
Soviet Union.

There is also evidence that both sides have attempted to avoid certain
actions that might be misinterpreted by the other side. In advance of sizable
military maneuvers--both by NATO and by the Warsaw Pact armies, for
example--both sides have been careful to "telegraph" their intentions in a
way that, if incorporated into a formal agreement, would be called an
"advance-notice agreement."

Suggestions for advance notification of missile launchings, aircraft
flights in polar regions, or even weapon tests have already been incorpo-
rated in certain formal proposals for arms control. But any usefulness of
these ideas clearly does not depend on their application through interna-
tional organizations, formalized procedures, and binding agreements. If
in practice both sides observe and implement the spirit of these ideas by
some kind of de facto advance notice or deliberate intelligence leaks, we
are engaging in mutual arms restraint. In answer to the criticism that
such informal procedures lack adequate "controls," we must say that in
almost no system of disarmament envisaged (except one imposed by force)
are direct controls likely to be the restraining force. The most important
requirement is invariably some form of self-control, induced by reciprocity
and sanctioned by the threat that the agreement will break down if re-
straint is not practiced. Even a system involving formal penalties will
still depend on self-control induced by the fear of penalties rather than on
direct physical compulsion. It is not even semantically inaccurate, there-
fore, to refer to informal restraints as forms of arms control.

Limited war itself is clearly a form of arms control. In fact, in some
ways it is a more dramatic form of arms control than restrictions on
preparations for war since it consists in the application of restrictions on
the practice of war. Again, it is evident that the absence of formal agree-
ments on the limits that can be, or have been, observed in a limited war
does not prevent these limits from operating. And, again, this observation
largely stems from the recognition that, if one side can do something, the
other can also do something and both may be better off in observing the
rules and in abstaining from certain forms of violence.

The U-2 incident is another reminder of the tacit controls that are
now in force. After this incident in May 1960. the United States called off
future U-2 flights. While one reason for this decision was undoubtedly to
avoid antagonizing third countries, the decision was probably also based on
the recognition that the continuation of the flights might provoke some kind
of Soviet reprisal. The Soviets had indeed threatened drastic action against
foreign bases from which the planes took off. Although one may doubt
whether they would have detonated nuclear weapons on bases in Turkey or

---

*To identify restraint or inaction, when it is conditional on an oppo-
nent's restraint or inaction, as "arms control" is of course not to identify
it as necessarily desirable. There can obviously be unwise and unfortunate
arms restraints just as there can be unwise or unsuccessful military or
foreign policy.

Pakistan, one can imagine other types of countermeasures or reprisals or dramatic demonstrations that would have constituted aggravations of the arms race or acute forms of military mischief.

One may wish to argue that the U-2 flight was "illegal," and that we cannot take credit for "arms control" when we simply abide by ordinary canons of international behavior. But we may get insight into the operation of restraints on activities and preparations if we recognize that international law is, to a large extent, an institutionalized understanding of the concept, that "What we can do, they can also do, and we may both be better off by abstaining from certain activities " In other words, the observance of some rules of international law is arms control of such long standing that we no longer recognize it as such.

One can easily imagine the breakdown of the normal rules of "peace-time" behavior, with ships violated at sea, sabotage within national bound-aries, tourists molested and captured, and even occasional mischievous violation of frontiers. It is indeed a remarkable commentary on the potency of existing restraints that so many traditional "international" laws continue to be observed between East and West during a cold war in which mutual trust is exceedingly low, threats of violence are common, and even general war is not out of the question. The existence of international legal rules can reinforce the inclination to observe these restraints if in the first place they are seen by both sides as mutually beneficial to their interests.

And it seems reasonably likely that in outer space some of the tradi-tions of maritime law and other rules may be applied by tacit decisions and agreements in view of the tendency to reason by analogy. While such rules will not automatically hold, we and the Soviets are likely to develop some practices on the non-molestation of each other's activities and even some rules prohibiting certain activities. It seems a fair prediction that these rules will develop through a process of tacit bargaining, of trying and test-ing, of observing and responding rather than through formal international negotiation. If so, this will be an important area of future "arms control"; and, by the study of the informal understandings and mutual restraints that are presently in force, we may develop a body of theory that will help to guide, strengthen, or avoid these space restraints as they develop.

As far as I know--and I have made some effort to find out--there is no systematic work going on to "take inventory" of the arms controls that presently exist. The rules that are embodied in treaties, agreements, and legal tradition, are surely well understood; but beyond those formal rules there must be a vast potential list of understandings, expectations, re-straints, latent but unutilized threats, norms of behavior, and even notions of "appropriate" reprisal that not only have never been codified but have never even been collected and examined from the point of view of arms control. The kinds of mischief from which we and our potential adver-saries abstain must be, for practical purposes, endless; but a sizable roster could be developed and subjected to study. The purpose would not simply be to count our blessings, though that may be worth doing. Seeing that some existing controls do not dissolve away may be as important as adding to them in the next decade. And the processes through which arms controls develop can be better understood by an analysis of those that have already been arrived at.

It is important, too, to avoid controls that, despite some mutually bene-
ficial effects, are on balance disadvantageous to us. A better understanding
of how we get into informal understandings may help to keep us out of the
wrong ones in the future, or at least to improve the understandings we
reach. The process by which a three-year de facto reciprocated ban on
nuclear-weapon tests came about is itself a fascinating study of the evolu-
tion of restraints and inhibitions.

For all of those interested in the process by which international rules
of behavior evolve or are created, an imaginative sample inventory of
existing arms controls, particularly of those that we customarily fail to
recognize as such, would be not only a great help but an ideal starting
point.*

Theoretical Models of Arms Races

It is an extraordinary fact, and a commentary on the organization and
motivation of the social sciences, that among the many types of feedback
models, interaction processes, interdependent-decision models, and equili-
brating mechanisms that have been studied, elaborated, computerized, and
articulated with the help of mathematics, cybernetics, and information
theory, the most backward is the study of arms races--the interaction
processes in military preparation. A few decades ago, Lewis F. Richard-
son (36) developed some theoretical models that, at the time, constituted
remarkable and advanced work in the social sciences. In the decades since
then, sociology and social psychology have gone a substantial distance in
developing interaction-process models, and economics has made enormous
leaps to the point where Richardson's arms-race models are well behind
the techniques of analysis presently available. (Computer simulation opens
up further research possibilities for present-day analysts.) Also in the
interim, operations researchers have developed extremely complicated and
sophisticated models of combat,** and explored complex multivariate
military-decision problems involving the allocation of resources and the
design of weapon systems (see McRae, pages 188 to 224 above). But hardly
anyone has brought to bear on the profoundly important subject of military
interaction in peacetime--arms races--the kinds of techniques that are now
taken for granted in several of the social sciences.

Anatol Rapoport (35), in a recent book explains the work of Richard-
son, puts it in a useful context, and builds on it a little. Kenneth Boulding (5)

---

*For useful studies on existing arms controls, see, for example:
Jessup and Taubenfeld (22); Taubenfeld (43); Halperin (18); Morton (32);
and Fisher (17).

**For those unacquainted with the literature, a nice sample of related
items would be Lanchester (26); Morse and Kimball (31, pages 61-80);
Engel (15); and Weiss (45). The literature of applied zero-sum-game
theory in models of combat is impressive; an example is Dresher (14,
pages 145-156). Non-zero-sum models of "limited combat," like models
of peacetime arms competition and buildup, are extremely rare.

The little classic by Ardant Du Picq (1) published originally on the eve
of the Franco-Prussian War, makes one acutely aware that combat, par-
ticularly before the era of battleships or push-buttons, took place among
individuals, not among nations. When Hannibal met Maximum Fabius it
was an N-person, not a two-person, non-zero-sum game.

carries it somewhat further. Arthur L. Burns (8 and 9), in an article that has received little attention, attempted to carry on this theoretical work, as has the writer (Schelling, 39, pages 209-229). Many military systems analysts are working on the potential ingredients of such a model, and a few implicit models have been used in discussions of the stabilizing or destabilizing effects of secrecy and inspection in the arms race. But the state of the art of models to analyze the arms-race process can only be described as lamentably backward.

Theoretical models can serve at least three useful purposes. One is to provide a conceptual basis for much semi-theoretical discussion that is now taking place. A good example is the role of secrecy--the elimination of secrecy about arms inventories between major powers can affect the tempo of the arms race in ways that can be usefully discussed in terms of information feedback and response. The influence of research and development lead-time, and the pace and cost of the arms race, can be better discussed with the help of models that involve timelags. We need better theoretical models because we engage in a good deal of loose, unsystematic, and verbal theorizing of a kind that, if it is worth engaging in at all, is worth doing properly.

Second, many choices among weapon systems and many analyses are made today, even by sophisticated organizations, that take little account, if any, of the differential influences of different weapon systems on the enemy's own weapon choices. A good example is the discussion of the relative merits of large, small, hard, soft, dispersed, mobile, or hidden missile systems. Since the "vulnerability" revolution of a couple years ago, when everybody became conscious of the crucial importance of invulnerability of our strategic weapon systems, many analysts have been engaged in computing the relative second-strike potentials of different configurations of missiles that might be purchased with a single budget. For a given assumption about enemy missile inventory and doctrine of employment, and for given cost assumptions, one can calculate the relative retaliatory potentials of these different kinds of weapon systems, as well as their "first-strike" potentials. Specifically, for certain assumptions, soft missiles may look better than hard missiles, particularly if the cost of hardening is great, and if enemy yields and accuracies reduce the protective effects of hardening and require the most expensive kind of hardening.

However, the soft and the hard missiles have very different implications for the arms race. If, instead of assuring fixed enemy inventories, we use the "interaction model," we have to consider not just our own decisions but also the enemy decisions. And the enemy decisions will be affected very differently by our choices of either hard missiles or soft missiles. Of course, an explicit model would also have to take into account the ease with which these decisions by one side are communicated to the other--the secrecy that surrounds the decisions, the enemy's understanding of their implications, and the possibility of a timelag in their communication. Whether we look at total numbers or total costs, a given goal of "retaliatory potential" may be achievable by both sides at a lower level of weapons, and very likely a lower level of cost, if both sides elect quality rather than quantity--that is, if both elect configurations that reduce individual-missile vulnerability rather than those that rely on large numbers to absorb the attack.

The matter is complicated, involving not only the usual characteristics of weapon systems—yield, accuracy, reliability, hardness, mobility, and such—but also involving production lead-times, construction lead-times, intelligence lag, accuracy of production forecasts, and numerous other factors. If arms-race models were given as much attention as weapon system analysis has received in the last several years, we would be in a much better position to give due recognition, in our budgetary decisions and weapon-system designs, to the interraction between our choices and the enemy's choices. In effect, the theoretical development of arms-race models and their application to current technical knowledge are nothing more than the extensions of familiar kinds of systems analyses to the arms competition on both sides simultaneously.

A third advantage of explicit construction and analysis of arms-race models is that we could learn a great deal about those arms control. that would tend to dampen or stabilize the arms race and those arms controls that, in overall effect, would tend to aggravate the arms race. In studying present and projected weapon systems we want to discover which of their characteristics are potent in determining whether the arms race is explosive or damped, in determining whether an equilibrium occurs at a low or high level of armaments, in determining whether there can be more than one equilibrium level, and, finally, how to identify these parameters with weapon characteristics, climate, intelligence, etc. Most conceptions of stabilized deterrence involve theoretical concepts which, in order to be discussed seriously and systematically, greatly benefit from a better conceptual framework, a better understanding of the kinds of interdependent-decision feedback models that arms controls are trying to modify or exploit.*

Needless to say, there is more required here than pure model-building. Identifying the parameters of a model with the data of a real arms race is a large part of the work. There may be important institutional factors that are difficult to work into a model. And not all models should be exclusively concerned with big-power strategic arms races; small-power arms races may be equally in need of systematic study with the help of explicit theoretical models.

Non-International Arms Control

Arms control is not exclusively international, and, in our model-building, we may get useful ideas if we look into the practices of the warfare that occur at other levels of society. Racketeers, delinquent gangs, racial groups, rebellious groups, even the police as well as the underworld all rely on the use and threat of violence to impose their wills on their enemies or in self-defense. Some of these groups engage in organized aggression and defense, using firearms and explosives; they engage in reprisals and threats of reprisal against "non-military" targets: they engage in alliances, truces, conventions to outlaw particular weapons and to neutralize certain areas, and sometimes "disarmament" arrangements with third-party supervision. They worry about the threat of surprise attack,

---

*Since this was written (in 1961), a first-rate example of systems analysis involving arms-race and arms-control behavior has appeared; see Kent (24).

blitzkrieg, massive retaliation, internal subversion, espionage, spoofing
and harrassment. Sometimes ethical considerations affect their actions.
They display the phenomena of cold war, limited war, surrender, "im-
perialist war," "holy war," and undeclared war. For the most part they
lack, as nations lack, institutional procedures for the enforcement of con-
tract in matters of war and peace, since most of these groups operate out-
side the limits of organized law.

I am not contending that what looks like "arms control" among delin-
quent boys is the same phenomenon as international arms control or that
its observable characteristics and lessons can be immediately transferred
to the international arena. But I would argue that this kind of warfare or
violence, and the development of limits and restraints on violence at this
level, can profitably be studied.

More confidently, I can assert that war and arms control at this non-
international level have not been given adequate systematic attention.
There have indeed been studies in the "causes of war," such as poverty,
broken homes, racial inferiority, inadequate police forces, police violence,
cultural traditions, and so forth. But there have been few careful studies
of what might be called the strategy and diplomacy of organized v. lence,
either as a phenomenon interesting in itself or as one suggestive of hypoth-
eses at the international level.

As previously suggested, our theoretical framework for considering
war, arms races, arms control, and the whole question of violence in in-
ternational affairs is still far from satisfactory. This is true only in part
because faulty analysis has excluded concepts and ideas that should have
been retained; it is primarily true because alternative conceptions and
ideas have not been perceived and explored. Ideas have to occur to people
before they can be examined, modified, and incorporated into an ongoing
body of knowledge. Stimulation and provocation are necessary to this
process. Analogies can be helpful, not because they prove something, but
because they provide ideas which, once recognized, can be examined on
their merits and justified without reference to the analogy.

Even if international war and arms control are quite different phenom-
ena from the non-international sort, we can hope to get new concepts, per-
ceive new distinctions, discover new possibilities, and identify new processes
by looking at war and arms control in such areas as the underworld or
gang warfare. Some recent work by Elton McNeil (30) is a good example.
It does not matter how one gets a good new idea, as long as one gets it.
Whether the source of this idea provides evidence for its applicability to
the international field is a different question.*

Public Opinion

Not much is known about popular conceptions of arms control. In view
of the extent that the arms-control policy of the United States government
is oriented toward public opinion, strikingly little is known. We lack sys-
tematic appraisals of what forms or levels of public opinion are considered

---

*I am supported in this by a fine statement in Wolfer's recent book
(47, pages xiii-xvii).

important, and we have inadequate knowledge of the manner in which public opinion is formed and informed on arms control, both here and abroad.

There are policy choices to be made that should be based on a better knowledge of opinion. Official United States positions on disarmament for the last several years, including President Kennedy's speech to the General Assembly of the United Nations on September 25, 1961, appear to have taken for granted that "world opinion" requires our commitment to, and emphasis on, something that has come to be known as "general and complete disarmament" (or just "GCD" to the trade). Many analysts have questioned whether GCD is a sensible objective. Part of the discussion has been concerned with the merits of GCD relative to other approaches to disarmament, but the Administration's positions seem to have been much influenced by its estimate of what world opinion demands.

For two reasons, it can be conjectured that we know disturbingly little about world opinion regarding matters related to arms control. One is that the arguments that assume the importance of the demands of world opinion are usually so vague as to what opinion matters that they imply a lack of criteria for judgment. The second is that the response of world opinion to alternative approaches has not yet been tried.

In view of this situation, three lines of study are suggested. The first is to discover more about the nature of world opinion on arms control: what different groups in different countries know about arms control, how they think about it, what they expect, what they hope for, and what they approve and disapprove.*

A second is a better evaluation of whose opinion (within countries, as well as among countries) should matter to us and of what kinds of opinions should concern us. (It is not necessarily a country's "good opinion" that we want whenever we take its opinion seriously.) Probably "opinion" is too narrow a word: beliefs, understandings, misunderstandings, expectations, and anxieties--and how all these relate to political action on the government or by the government--are what we need to know more about. This part of the subject, however, goes far beyond the matter of arms control.

A third line of study is to look at the process by which opinion is formed or informed. Who has to be persuaded first in order that large bodies of opinion be changed? What is the role of newspapers and other media? (It is often argued that anything more complicated than general and complete disarmament is beyond the comprehension of "the people." But is it beyond the comprehension of Washington correspondents of foreign newspapers? If foreign newspapers begin to comprehend this somewhat complicated subject, might not they in turn influence larger blocs of opinion?) How much is opinion influenced by the attitudes of leaders who can devote attention to the problem and have the intellectual ability to make the distinctions, evaluate the alternatives, and understand the approaches that rest on analytic studies?

Direct study of opinion may be helpful, but indirect studies may be more effective. What people know, what distinctions they make, and where

---

*A good example is the work reported by Lerner and Gorden (28).

they get their information may be as important as what opinions they hold.
It may be useful to determine (a) how much their opinions on disarmament
depend on American statements and actions on disarmament and (b) how
much on its statements and actions in military policy and other fields. In-
vestigation could help to show whether opinion about American policy is
closely related to the policy that is actually announced and implemented.
Finally, it should be established whether adverse opinions about American
arms-control policy are held by people whose opinions of the United States
are in any case adverse, or whose opinions are formed through information
media that are adverse, because it may be fruitless to try to accommodate
those attitudes by our approach to arms control.

There is a remarkable tendency for policy-makers to get their infor-
mation about world opinion by talking to each other. Even genuine sources
of information appear to be biased—the United Nations community in New
York, particular newspapers, visitors' observations in foreign countries,
etc. Foreign representatives here must usually be regarded as poor
judges of the current opinion trends in their own countries unless system-
atic investigation is continually undertaken. What these representatives
tell us often has an exaggerated influence on our official estimates of
opinion abroad. Greater efforts in public opinion research if coupled with
an interest in using that research whenever policy is adapted to public
opinion might help offset these tendencies.*

Soviet Attitudes Toward Arms Control

Official Soviet attitudes toward arms control are critically important
to the development of our arms control policies, if we hope for fruitful ne-
gotiations, but again we are remarkably ignorant of what these attitudes
are.** I had the privilege of attending the conference on "Disarmament
and World Security" in Moscow in December 1960 (the "Pugwash" confer-
ence), and, since that conference, I have been amazed to find myself con-
sidered an authority on Soviet attitudes toward arms control, even by pro-
fessional students of Soviet policy. This is about as satisfying as being
told, upon climbing into the dentist's chair, that I know as much dentistry
as the dentist. Our evidence of official Soviet attitudes is flimsy.

There is a complicating factor. Many students of Soviet policy make
inferences about Soviet attitudes that result from a combination of direct
evidence and logical reasoning. One is often, for example, confronted with
the following type of argument: The Soviets would clearly prefer a world
in which the absence of war were guaranteed, and, therefore, the Soviets
would clearly prefer total disarmament to the present arms levels and
security systems. This argument depends partly on a Soviet preference
for the exploitation of a "peaceful" world; it depends partly on a Soviet be-
lief that total disarmament would eliminate war. Thus, even Sovietologists
often mix their analyses of Soviet doctrine with their own strategic
thinking--their judgments on the sort of world that total disarmament

---

*A discussion of research on public opinion related to general prob-
lems of national security appears on pages 46-74 above.

**For existing literature on Soviet attitudes, see Mackintosh and
Willetts (29); Barnet (2, and 3, part II); Nogee (34); Bechhoefer (4); and
Dinerstein (12).

would produce.* This confusion is probably inevitable because of the scarcity of evidence on Soviet attitudes at the official level.

I assume that much is being done to examine overt evidences of Soviet official interest in arms control, certainly through analyses of Soviet statements on the subject, and in this field I have nothing to propose. But there may be some important things we can do indirectly.

First, we can learn more about the material on arms control published in this country and in other Western countries that Soviet policy-makers read. How soon do they read it? How well do they understand it? What types of material are they most likely to cite or to quote internally? What are the technical and ideological obstacles to good translation? How can concepts be made more comprehensible to them and more acceptable to them? How much is the apparent Soviet intransigence on arms control a matter of lack of knowledge, intellectual backwardness, or an inability to rationalize desirable policies as consistent with communist doctrine?

If arms control is ever to be taken seriously, improved communication between East and West may be required. How good is communication on strategic concepts and arms control already, and are the useful Western ideas on arms control quickly translated and appreciated? Or is a more conscious effort required to feed notions and concepts to the Soviet hierarchy in order to prepare the way for possible agreement? Do we have an intelligence program that tests the efficacy of certain media of communications, the relative impact of different authors, and the relative merits of formulating and publishing ideas in this country or in particular other countries?

The Process of Negotiation

Most of our foreign policy entails some kind of negotiation, formal or informal, tacit or explicit, bilateral or multilateral. It hardly seems necessary to suggest that arms control, too, should provide a special motive for examining the process of negotiation. Yet it seems to be true that over the last several years we have explored and experimented with negotiating techniques in the field of arms control and still know little about negotiating with our adversaries. We have developed some concepts, like the distinction between "political" and "technical" discussions. We have gained some experience with the role that can be played by scientists who are not normally engaged in the diplomatic process. And we have had a novel opportunity to negotiate with adversaries on subjects in which sensitive security information was involved.

---

*The same mixture of external evidence and internal reasoning occurs often in professional analysis of the Chinese attitude toward general war. Attribution to Chinese Communist leadership of a great willingness to risk general war (or to the Russian leadership of a belief in Chinese willingness to risk general war) appears to coincide with a belief that the Chinese Communist system could survive general war comparatively well and hence that Chinese Communist leadership has good reason to incur greater risks than other nations. In this relation see Coale's discussion, pages 169ff above.

Nevertheless, in spite of centuries of diplomatic experience, we are still fairly ignorant about the process of negotiation. There is little theoretical work available in the social sciences that helps even to characterize, much less to analyze, the kinds of negotiations we have had on surprise attack, test-ban, or general disarmament in the last few years.

There are at least two distinctive types of negotiation that deserve separate consideration. One is the large and formal diplomatic encounter of the kind that has been taking place at Geneva. The other is the much more informal, tacit, and less self-conscious kind that occurs when we send up a satellite, make an announcement, and wait to see what the Soviets will do or say. The latter was referred to in the earlier discussion of tacit arms control; it is a process that needs to be better understood, and for which we need more imaginative theory.

Even the formal diplomatic negotiations, though, are remarkably immune to systematic analysis. There are some good reasons for this, one of which is that these complex verbal policy engagements among countries somewhat defy analysis. Another is that professional students of the negotiating process are generally excluded from actual negotiations, so the raw materials for analysis are not directly available to those who might bring their professional disciplines to bear. The disarmament negotiations do have, nevertheless, the analytical advantage that they involve enemy countries, and for that reason important segments of them cannot be highly classified. While the American position and reasoning may be classified, the transcripts of these conferences are often available and do provide much of the material for analysis.

Nathan Leites, now of the University of Chicago, recently devoted several months to the detailed perusal of the transcripts of three of these negotiations--the test-ban, surprise attack, and ten-nation general-disarmament negotiations. In an imaginative way, he examined the negotiating techniques and roles on both sides, Eastern and Western, and contrasted the two. His observations were highly provocative and exceedingly valuable. But this is only a beginning, and he lacked access to the negotiations themselves. While there may be advantages in working from the transcripts alone, uncontaminated by direct personal contact, there are undoubtedly other advantages in personally experiencing the negotiating atmosphere itself.

In the study of negotiations, one of the striking phenomena is the difficulty of deciding what the criteria are for evaluating the performance of our own negotiators. What is the objective of the negotiations? What are we trying to accomplish? What constitutes "success"? We may try to persuade the Soviets immediately or we may try to educate them for the long run. We may try to impress third countries or we may try to impress the allies who are our negotiating partners. We may try to establish precedents and traditions and procedures for future negotiation. We may try to convey to the Russians our national intentions and our positions on many subjects other than those under negotiation. We may try to meet certain procedural requirements. We may try to avoid being the ones to break off negotiations. The individuals involved may certainly try to score points, impress each other, alleviate their boredom, or conduct inter-allied negotiations in the course of what is normally an East-West negotiation.

We may be trying to get intelligence information through the negotiating process. And many other objectives may be present.

One suspects that there is no overall "strategy" of negotiation, at least for our side. Our own inter-agency differences alone may preclude any very coherent detailed strategy, and inter-allied differences also prevent it.

Furthermore, it often appears that negotiations have a certain momentum of their own. People are involved; they have some preconceived ideas on how negotiations take place, on the roles they play, and on the customs and rules that govern negotiations. Especially if in a foreign city, the negotiators develop a small community of their own that becomes obsessed with what is going on locally; they tend to forget the rest of the world and to exaggerate the particular negotiations they are engaged in. They are affected by some sense of contest, or some personal challenge, in their response to what goes on, and they seldom lack a pride of authorship. In short, there is individual behavior as well as national behavior to be examined.

Systematic study of the negotiation process may help not only to understand it but also to make better some choices that have to be made. How secret should negotiations be? Where do the "real" negotiations take place--in the site where the diplomacy is involved or behind the scenes; locally or far away in capital cities where ambassadors are still dealing with foreign offices and heads of government? To what extent can arms-control negotiations serve as a vehicle for conveying intentions on other matters to the communist bloc? How much personal involvement is desirable? To what extent should negotiators be rotated to prevent the establishment of personal relations? To what extent should the negotiation be aimed at building up a cumulative record, and to what extent at solving current day to day disputes? To whom is the negotiator really talking--the man across the table, that man's boss back home, the head of the negotiator's own government, or the population of his own country? How are negotiations terminated if they are getting nowhere and becoming an embarrassment? How much should our team help to build up the prestige and good will of the opposing team, or how much should our team try to embarrass the opposing team with its own government? How does one recognize and cope with diversionary or divisive tactics on the other side? How informal should our negotiators try to be, how candid, how cooperative? How does one tell whether a conference is getting somewhere or not? To what extent does a lengthy negotiation go through organic stages that can be classified and recognized, such as a warm-up or build-up stage, a stage of engagement, and a stage of rapprochement? How much attention should be given to local conditions and tactics like fatigue, needling, refutation, and informal personal contacts, and how much to the overall strategy?

These matters have undoubtedly received some attention from professional negotiators. My own judgment is that the best artists are not necessarily the best analysts and that intensive personal involvement is not the best basis for analysis. Arms control over the years and decades to come is likely to involve a great deal of negotiation of many different kinds; a

better understanding of the purposes, techniques, consequences, and mech-. anism of these negotiations could be exceedingly useful.*

Administration of Arms Control

While it is important to emphasize that many arms controls require no formal agreements, no formal institutions, and no formalized procedures, it is nevertheless true that many kinds of arms control (including all the arrangements that have been officially proposed) involve some formally established institutions. These institutions will have procedures, personnel, budgets, and governing bodies, as well as political relations with signatory nations.

Some administrative questions have received a good deal of attention. The "troika" is an example, and the problems following the death of Dag Hammarskjold dramatized the depth of East-West disagreements over the organization of the United Nations and, by implication, of all international organizations involving Eastern and Western participation. The most important issues are undoubtedly those of political control. No important international organization responsible for matters as critical as arms control is likely to be a purely "international," independent administrative unit. It will in some sense represent or be responsive to the signatory nations and raise questions of political control at least as complex as those that arise within nations.

In addition, there are questions of administrative arrangements that deserve much more careful attention than they have received. Some of these have already arisen in connection with the test-ban. A good example is what information is kept within the organization and what is passed along to signatory nations. Another is the national-personnel make-up in the various organizations. An issue that has not received adequate attention is the budgetary problem. There may be numerous problems that have been overlooked simply because great matters of principle have kept our attention from mundane but critical issues that await us.

Many arms-control arrangements will be expensive. A test-ban detection network itself could cost billions of dollars. Budgetary negotiations would be difficult if nothing but money were involved; but where sizable amounts of money are involved and "legitimate" financial issues can also be raised, deliberate obstructionism is an important possibility. Analogous instances appear to occur in the recurrent United Nations budget crisis where budget obstructionism seems to be used to register disapproval of United Nations activities and even to sabotage them. If we ever reach a stage of arms control where some international military authority has to be maintained and financed, budget negotiations may become an important arena in which matters of principle are argued as though they were financial questions. Financial objections could become useful substitutes for political objections when the latter might be embarrassing.

Many ambitious schemes for disarmament envisage an international force that has a near monopoly on weaponry and modern military technology.

_____

*On the negotiating process. see Zoppo (49); Dennett and Johnson (11); Wolfers (48); Nogee (33); and Douglas (13).

Some proposals suggest that national forces be modeled on our domestic police, with no equipment beyond squad cars and side arms, while the international "peace-keeping machinery" be provided with ships, aircraft, submarines, and possibly nuclear weapons.

In addition to budgetary problems, these schemes would raise questions about the _location_ of production of military equipment for the international forces. The possibility that some countries might achieve an advantage by renouncing the agreement might be much affected by the distribution of immediate military productive facilities among existing industrial nations.

If all the arsenals and shipyards of the international force were located in an internationalized Antarctica, such problems might not arise. But if an international force contracts for all its complex weapons and other paraphernalia of military control with firms located in the industrialized countries, or if it establishes arsenals for these end-items within the countries most capable of producing them, there will be a certain latent productive capacity in each participating country that represents not the assets it owns but those that are _located_ within its borders and in some sense subject to its physical control. Not only will the allocation of military production be a matter for highly sensitive negotiation, but also the strategic significance of such military production will necessarily be part of the disarmament environment.

These and many other "administrative" questions would be waiting to engage us if we could surmount the more immediate political-military questions. Meanwhile, these "administrative" questions should be getting somewhat more attention than they have received. Even the test-ban negotiations, which brought us near the brink of some formalized arms control, produced remarkably little detailed analysis of administrative questions, despite the almost unique simplicity of the test-ban as a form of arms control. What constitutes a nuclear-weapon test is comparatively simple to define, and the proposed ban was likely to be an exclusive one or, at least, one involving qualitative prohibitions rather than one setting quantitative limits. With weapon limitations rather than exclusive prohibitions, the greater degree of administrative complexity would seem to deserve much more attention.

Much of the discussion of the administration of weapon limitations has assumed that there would be a fairly black and white distinction between compliance and evasion, with inspectors appointed to find illegal clandestine violations. That this assumption vastly oversimplifies the administrative problems can be illustrated by our experience in this country with enforcing certain controls on our military services. We have had limitations imposed by Congress, by the Budget Bureau, by inter-agency agreements, and by executive order. But we have had actions that stretch the rules, distort the rulings and interpretations, and exploit the loopholes. The Budget Bureau does not send around the FBI to get evidence of illegal evasion, and then institute legal proceedings to convict and punish the violators. Rather, the Budget Bureau, or the Civil Service Commission, or the General Accounting Office, or a Congressional "watchdog committee" looks for evidence of some kind of quantitative excess, argues about what is legitimate, restates the prohibitions, imposes direct and indirect sanctions, occasionally causes an administrator to be replaced, threatens

reduced appropriations, and engages in the perpetual process of trying to enforce personnel savings, budget ceilings, jurisdictional lines, and so forth.

The same might well be true under arms-control agreements. An agreement on military manpower will lead to arguments over the functions that can be performed by civilian employees in substitution for uniformed personnel. Limits on numbers of weapons will become involved in problems of spare parts, replacement rates, test and development weapons, in-commission rates, pipe-line, and many others. There will be arguable lines between military activities and non-military activities in outer space, in research and development, and in internal security. There will be arguments about the designing of ordinary civilian equipment so that it has some war potential or conversion capability. There may well be a good deal of "evasion" by nations who push the limits of the agreement, "nose around" for loopholes, and generally do what any military service does when it finds itself subject to personnel limits, budgetary limits, or statutory limits. And, in all of this, there is the possibility of drummed up charges, accusations of abuse, competitive violations, and so forth.*

Just how to anticipate and prepare for these types of "evasion" and to design institutions that can manage them is going to be difficult at best and undoubtedly highly pragmatic. There are, however, areas of study that may help in our preparation. One would be to study the avoidance (and alleged avoidance) by the armed services of this country of limitations imposed on them with which they did not agree. These could be personnel limitations, allocations of roles and missions, devices to get around conflict-of-interest rules, and so forth. Another useful area of study would be to analyze relations between the United States and its allies, or between the United States and recipients of military assistance, on the issue of enforcing past arms agreements. Many of those agreements provided floors rather than ceilings, minima rather than maxima. We have had to deal, for example, with the problems created by the failure of several of the NATO nations to make available the numbers of divisions promised by the Lisbon agreement of 1952. Our military relations with friendly countries may shed a good deal of light on our potential military relations with potential adversaries in an arms-control agreement.

Concepts of Inspection and Enforcement

It is widely taken for granted that inspection of arms control should be impartially administered by some international organization. Whatever its merits, the almost exclusive preoccupation with this concept has apparently arisen by default rather than by examination and elimination of alternatives. Fred C. Iklé (20) has pointed out that "partial" (i.e., "non-impartial") inspection of each other by potential adversaries makes just as good sense a priori, and may suit particular arms agreements (see also Finkelstein, 16). It is not out of the question, moreover, that inspection should include, as one of its premises, the idea that each side has an interest in providing adequate evidence of compliance, and that one of the judges of "adequate" inspection can be the party being inspected, which if innocent will want to establish its innocence.

_____

*Cf. Schelling and Halperin (41, pages 107-119, on "Regulating an Agreement").

In fact, the idea that inspection should be concerned with the establishment of innocence as well as with the establishment of guilt has received too little attention. In discussions of the test-ban, for example, there was almost exclusive preoccupation with inspection to find evasion in the event evasion takes place. There was little recognition of the enormous irritation that can result from inadequate inspection that is incapable of providing assurance that the agreement has been complied with, in the event no evasion has taken place. "False negatives" are not the only problem: "false positives" are also a problem. For one major power mistakenly to believe the other is cheating can be damaging to world security and arms control. There were suspicions in this country, for example, that Soviet tests were taking place during the test-ban negotiations, it seems likely that even after a formal agreement such suspicions would have continued if the inspection arrangements were felt to be ' .adequate.

It is hard to escape the impression t ·t the preoccupation with particular concepts of inspection has resulted . from an examination of several competing concepts and the selection of a p. icular one but from lack of acquaintance with alternative approaches to the problem, even from lack of an adequate checklist of considerations. It would help to have some theoretical framework on the inspection problem if only to provide a reasonably comprehensive statement of alternative approaches.

The development of a theoretical framework probably depends initiall, on encouraging more imaginative thinking. This could be stimulated by an exploration of the inspection problem as it arises in other very different contexts. Bank inspectors undoubtedly have some knowledge and techniques worth examining. Brink's and other companies that handle va uables must have special techniques for making organized robbery difficult: the random choice of delivery routes and other tricks of evasion, inspection by surprise, methods for personnel selection and for dealing with employee defection or conspiracy, etc. Methods of espionage and counterespionage are also worth examining, as are methods of illegal collusion in restraint of trade and antitrust activity. The relation of the police to the underworld, as well as the problem of corruption within the police force, may be worth studying.

Problems of inspection and detection have been with us for a long time, but no existing discipline seems to have organized the problem, surveyed customs and practices, or provided a comprehensive analysis and comparison of techniques, arrangements, and countermeasures. We may be overlooking ideas that would appear quite obvious if they once occurred to us. There may be, for example, techniques for trapping the unwary violator or techniques to facilitate the establishment of innoc nce--i.e., the establishment of alibis--for those who, though innocent, are under suspicion. There may be ingenious and valuable methods of detecting falsifications in record-keeping that would be familiar to those who have dealt with embezzlement or illegal price-fixing and market-sharing. There may be useful concepts of where to search for evidence of violation or how to spoil the fruits of violation, concepts that are well known in other areas but that have not yet occurred to students of arms controls.

It might be important, for example, to realize that a violation of an arms control agreement--a test-ban, perhaps--would be dangerous only to the extent that its fruits could be incorporated effectively and covertly in

military technology and doctrine. If the technical knowledge thus gained cannot be used without betraying the fact of the violation, it will be of little value. The technical knowledge that is incorporated in weapon technology, or even in military doctrine, may become observable in a way that the violation itself was not. (Counterfeiters are usually caught in their attempts to spend their money, not in producing it; spies are often caught not in spying but in communicating the information.)

Focusing on the links in the chain may also assist in dealing with the deterrence of violations and possibly with redressing the violator's advantage. Another country may successfully conduct clandestine tests and gain knowledge of certain weapons; but if, to utilize that knowledge, it has soon to incorporate it in doctrines, technology, and deployments that we can observe and study, that country may be forced to allow us to derive some late benefits from its clandestine violations.

There is a distinction between wanting to know that the other party has been violating an agreement, if he has been, and needing to prove the violation to some third party (including one's own population, political leaders, appropriations committees, etc.). Reliable knowledge and usable evidence can be quite different things. Which one is needed depends on the particular agreement and on what effective forms of recourse are available-- denunciation, application of penalties, or internal decisions also to resort to clandestine violation.

This reminds us that, in some instances, it is not enough to know that a violation has occurred; we may have to know who has committed the violation. Weapon tests again are a good example. A nation that has produced or procured a stockpile of presumably reliable nuclear weapons may simply want to know whether they work roughly as expected. When a sudden explosion of a nuclear weapon occurs in a far ocean, with a few unidentified vessels nearby, we may not know whether Chinese, Egyptians, Israelis, Cubans, Argentines, or others, have been involved.*

These remarks about the several roles of inspection and the several roles of evasion are meant only to be suggestive. But they raise the important question whether official thinking on arms-control inspection may tend to fall prematurely into unimaginative patterns and grooves. There is probably a good deal of uncollected and disorganized knowledge available among those who have been engaged in various aspects of the inspection business, and it is a failing of the social sciences that this aspect of administration theory, organization theory, or game theory--whatever we call it--has never been developed.

.s on the enforcement of arms-control agreements are closely tied in with concepts of inspection. Enforcement is presumably through some process of discovery and exposure, discovery and renunciation of the

---

*Many weapons are desired for threat purposes rather than solely for active use. Capabilities that must be acquired clandestinely may be less available for threatening, since threats depend on a demonstration of possession or capability. Thus, the incentives to commit certain violations may not be as strong if a country cannot exploit the threat aspect of a weapon's capability clandestinely acquired. .

agreement, or discovery and more elaborate reprisal. Inspection is sup-
posed not only to serve the purpose of discovering violation but also to
deter violation through the threat of exposure, renunciation, or reprisal.
It is not detection but the consequences of detection that may deter.

The entire concept of enforcement in arms control is in need of sys-
tematic and creative development. We have had much experience with the
enforcement of international agreements through legal procedures. We
have had some historical experience with enforcement of restraints and
proscriptions on small countries by large ones, particularly on colonial
areas by a metropolitan power. And we have some ideas on wartime and
peacetime deterrence through threats of military action. But there is no
good theoretical framework in existence for discussion of the "enforce-
ment" of arms control.

If we turn to other areas of enforcement, such as enforcement of
domestic laws, we are also quickly disappointed in the theories and con-
cepts that we find. There is a great deal of law and knowledge about the
enforcement of criminal laws, about enforcement of anti-trust laws, and
about enforcement of contracts. Though some of this has been nicely sys-
tematized in terms of legal reasoning, there has been remarkably little
use of economic theory, decision theory, organization theory, information
theory, or sociological theory in the development of a general theory of
information, deterrence, and enforcement.

In discussions of arms control, there has been much argument on what
probability of detection would be sufficient to deter, say, a decision to vio-
late clandestinely a nuclear test-ban. There are no adequate theories of
criminology, or economics, or decision-making that would help us to for-
mulate this question or to analyze it.

The problem is not that we lack a fancy and elaborate theory. It is
that we lack even a rudimentary theoretical framework for talking about,
for example, how and to what extent the incentives to violate are affected
by such factors as: the likelihood of detection, the likelihood that detection
will lead to punitive or costly action, the size of penalties, or the counter-
measures available. Nor have we a framework for discussing the tech-
niques of deterring the penalty itself, the problems of coping with the un-
certainty whether violation has occurred, the distinction between single-shot
violations that accomplish their purpose at once and those that have to be
continued, the distinction between inspections to discover the occurrence of
a violation and to discover the type of violation and its accomplishments,
the dangers and costs of false beliefs of violations, the relation of all these
things to timelags, and the relation of individual incentives to national
decisions.

To date, the best work in this area has been done by Fred C. Iklé (20).
Iklé's work should not be described as highly theoretical. However, his
systematic emphasis on relevant considerations, in such contrast to most
discussions of the subject, makes his work the nearest thing we have to a
beginning theory of enforcement and inspection.

Game Theory and Statistical Sampling

Arms-control inspection is likely to involve sampling procedures. Not
every establishment can be kept under continuous surveillance; not every

untoward event can be exhaustively investigated. Some system of spot checks, selective investigation, and search procedures will be required. This problem was highlighted by the test-ban negotiations in which the es-timated fraction of untoward events that should be subjected to on-site investigation became a main point of bargaining.

How best to use some quota of spot checks or on-sight investigations; how to compare alternative quota systems; and how to evaluate the efficacy of a particular quota system—these are questions that involve both sampling theory and game theory. In ordinary sampling, it is assumed that the universe stands still, or at least that it does not consciously adapt it-self to the investigator's sampling strategy. If we wish only to know the average speeds of motorists in different parts of the city, and if we have a limited number of inconspicuous mechanical devices for measuring the speeds of passing cars, any good statistics text will provide us with the theory necessary to allocate these scarce devices among a large number of street corners. If, instead, we wish to allocate scarce policemen among a large number of street corners to catch violators, ordinary sampling theory is inadequate. Motorists will adapt their behavior to what they know, or can guess or deduce, about the way we allocate policemen.

Similarly, if we had a detection network that was wholly unknown to the Russians, and could observe them secretly on a sample basis, we might utilize this network in accordance with ordinary sampling proce-dures. But if the Russians know that we have the network, they can dis-co' .r .: any statistics text the th ory we would ordinarily use in allocat-ing o.. .arce inspection resources. We a. ., then, at once involved in the problem of outsmarting a conscious adversary. Furthermore, if the Russians have direct access to our sampling procedures because they participate in the management of the surveillance systems, we need to use randomization techniques within the residual limits of Soviet ignorance.

A couple of examples will suggest the nature of the problem. First, suppose the Soviets wished to violate a ban on some kind of activity— nuclear-weapon testing, static testing of rocket engines, training of the military forces, military maneuvers, construction of missile sites. loca-tion of military production, or anything of the sort. If they assumed that they were not being subjected to sampling inspection, they would be guided in their choice of times and locations by costs, convenience, coordination with other programs, and so forth.

On our side, if we knew something about the relative costs of bases in the far north, in the desert, near cities and railroad lines, or in remote areas, or about the relative costs of weapon-test stations in different loca-tions, we could make some a priori guesses about the relative likelihoods of the use of different locations and of times of day or times of year. We would allocate most of our inspecting resources to the times and places that the Soviets would be most likely to choose for their clandestine viola-tions. If, however, the Soviets know that we have such resources and are using them on a sampling basis, they have reason to disperse the activity into areas, or times of day or year, that were not optimal in cost and con-venience but that would cor licate our sampling problem. Moreover, if they had som e knowledge of the costs and efficiencies of our inspection techniques in different areas or at different times of year, they could adapt their activities to minimize the efficiency of our inspection.

(Frequency of earthquakes and seismic characteristics of the ground have been important in test detection systems.) This kind of sampling procedure contains a strong element of "search and evasion" of the sort to which game theory is pertinent.

A second instance is the quota of inspections per period of time. A dozen inspections per year should be evaluated very differently if it means an average of twelve per year over several years or a maximum of twelve in any specified twelve-month period. Twenty-four inspections every two years is appreciably better than twelve per year, and the latter in turn is better than six per semester or one per month. The reason is that any fixed quota in a specified time period runs the danger of being exhausted before the period is up, leaving a dead spot in the inspection system at the end of the period. If a part of the quota has to be saved until the last moment, it may become essentially wasted (see Department of State publication, 42, page 233). The provision for "carry forward" and "carry back" can increase the efficacy of any given numerical quota.

Another consideration that affects one's sampling procedure is that the subject of inspection may be able to degrade the inspection system by the use of decoys or by otherwise spoofing it. To use a crude example, if one side had a way of inducing small earthquakes, they could reduce the sampling ratio involved in any particular quota. Or if they could anticipate earthquakes and focus a variety of suspicious activities on a few selected earthquakes--activities such as unusual communications traffic or air movement or some odd installations in the area of the tremor just prior to its occurrence—they could induce the inspectors to exhaust precious inspection rights on the dummy.

This possibility of creating noise and decoys requires empirical examination in connection with each kind of evasion. One has to look at the opportunities for evasion, the costs of such tactics, how the evasive mechanisms might interfere with other activities. and the danger that even these facsimile events would yield useful intelligence to the inspector. A theoretical model of the study of optimum sampling against a conscious adversary should make allowance for the latter's skillful use of noise and decoys, as well as for his skillful accommodation of his evasive activity to what he knows about the inspector's sampling criteria.*

Secrecy

Secrecy is an important political phenomenon. There is a good deal of official and informal secrecy in the conduct of official United States affairs. There is incomparably more in the Soviet Union, where official secrecy is pervasive and serves a multitude of official purposes. Secrecy evidently has numerous benefits for a totalitarian government, as well as

---

*This gap in our sampling theory was brought to my attention two years ago by Andrew W. Marshall, who reported that a search through the literature on sampling indicated that little had been done to deal with this problem of the consciously evasive adversary. Since then Melvin Dresher, a professional game theorist at RAND, has initiated some work on the problem. The Arms Control and Disarmament Agency reported in 1962 a contract for research in this subject.

a great many drawbacks. Most kinds of arms control are in conflict with
th? entire phenomenon of official secrecy.

.   There are exceptions. It is not out of the question that an arms agree-
ment could provide for the preservation of secrecy of certain sorts, so
that the security of strategic systems would not be jeopardized, excessive
targeting information obtained, and military vulnerab'''''es created through
excessive intelligence. In principle, this is an important exception, but on
balance there is no question but that the progress of arms control would
be impeded by considerations of secrecy. Moreover, the institution of
arms-control arrangements, in requiring specific elimination of secrecy,
may tend to undermine the entire institution of official secrecy.

The progress of arms control may thus depend on a trend in the di-
rection of lesser official secrecy, particularly in countries that rely on
pervasive official secrecy. (We can also have, as one of our motives for
arms control, the erosion of secrecy in communist countries.) It is easy
to find specific points o' which arms control impinges on secrecy, and not
too difficult to evaluate the immediate or localized costs and disadvantages
that a country would suffer in losing some of its secrecy. This conse-
quence of arms control deserves more attention than it has received, al-
though it does not raise problems that have not been appreciated.

Official secrecy in a communist country may be more than simply the
aggregate of individual secrets, some of which are in conflict with arms
control. Secrecy may be an attitude, a philosophy, a political principle, a
tradition, and a part of the entire social fabric. The enforcement and
maintenance of secrecy may depend on attitudes, norms, and practices that
would be eroded by any large-scale elimination of secrecy. A government
might look negatively at ar; arms control scheme on the grounds that the
political acceptability of secrecy within the country might be jeopardized.

Thus the implications for official secrecy of ad hoc arms-control ar-
rangements may go beyond the particular requirements of the arms agree-
ments themselves. Beyond that, there is a possibility of a general attack
on secrecy itself, with arms control as spearhead. Edward Teller (44) and
others have proposed that an attack on official secrecy within the Soviet
bloc may be a prerequisite to the ultimate "civilization" of these govern-
ments and to the establishment of decent peaceful relations between East
and West. Many have suggested that the breakdown of secrecy is a legiti-
mate objective of arms agreements. And some have suggested that the
sheer incompatability between secrecy and arms control provides a propa-
ganda opportunity to the West, which can use arms-control proposals as a
way to discredit secrecy within the Soviet bloc or at least to discredit the
Sc iet bloc among countries that find official secrecy uncongenial.

If arms control is a ,   ible technique, then, for undermining official
secrecy w.th.n the communist countries, as such it is also a potentially
effective tool with which to influence the political development of the con..
munist bloc from the outside. For this reason and for progress in promot-
ing arms control in the face of the obstacle of secrecy we could benefit
from a careful and systematic study of the political and social role of of-
ficial secrecy in totalitarian countries, particularly in the communist bloc.
While much has been written on the subject, our analyses are insufficient
in a number of instances: on the values and techniques of secrecy, on the

political and bureaucratic conditions of secrecy, and on the impact of official secrecy on individuals and particular institutions in communist countries. Improved research in these fields could bring us closer to an overall evaluation of what arms control is up against or what it may accomplish.

Techniques of Personal Coercion

Another important and fascinating aspect of the international power struggle that has been largely neglected is the coercion of individuals rather than states. The current heightened interest in "paramilitary" activity, "sublimated" war, subversion, revolt, and guerre révolutionnaire should help to stimulate study in this field.* There are also good grounds for its consideration in a discussion of arms control, for the coercion of individuals (along with weapons of propaganda, economic competition, and diplomacy) is one of the possible techniques of international violence in a substantially disarmed world.

It has been reported that in Vietnam the assassination of public officials has at times occurred at a rate of over a hundred per month. Earlier, it was reported that the East German government was developing a list of West Berlin officials who would be subject to severe retroactive penalties at such time as the city of Berlin came fully under communist control. Violence and threats of violence against individuals, particularly those against key individuals or persons essential to local leadership and local communication, constitute a little recognized form of "warfare," although, in areas where active revolt is taking place, terrorism receives more notice (see Crozier, 10).

In a disarmed world, personal intimidation may assume even greater importance. Personal intimidation is somewhat akin to bribery or the offering of rewards to defectors and saboteurs. These are "positive" inducements to individuals, while threats of violence are "negative" inducements. Both are ways of influencing the behavior of governments or nations through action at the individual level.

The restraint and limitation of warfare against individuals may therefore deserve to be the subject of arms control research, which has so far concentrated on warfare at the national level.

Lead-times in Disarmament Planning

An interesting series of questions relates to lead-times on weapon systems. If we were to incorporate arms restrictions in our weapon systems, how long would it take to incorporate them? If we wanted to impose a moratorium on the development of certain weapons, in order to freeze designs and to settle on "present" types or numbers of weapons, how would we define "present" weapon systems? How would we identify "existing" technology? And how long would it take to design bases, communications, and ancillary facilities to match the agreed weapon types or numbers?

---

*A discussion of internal war appears in Eckstein's study on pages 102 to 147 above.

If, for example, we were to agree right now (at any given date) to maintain present forces and weapons, what would those forces and weapons be? How committed are we—in basing concepts, training, communications, and forward procurement contracts, etc.—to the forces and weapons of some future date? In general, how could we handle the dynamics of weapon systems in a disarmament program, and how would we handle the phasing?

The general questions here are two-fold. First is the problem of lead-time narrowly defined; what is the lead-time for conception, development, procurement, and training for specific weapon systems and families of weapon systems. Second is the problem of adaptation of the military establishment as a whole to the sometimes revolutionary impact of a new concept and the developments coming from it. The most conspicuous case, perhaps, is the atom bomb, but the airplane, the missile, the rifle, radar, miniaturization, and other examples come to mind. Each has, or is, forcing a major reorganization of the military establishment as a whole.

Some interesting work has been done on lead-times for specific systems, notably by economists at the RAND Corporation. Herman Kahn (23, page 316) has proposed that a revolution in weapon systems takes place every five years and that the lead-time on major weapon systems is something like ten or fifteen years. Cycles in the development of new technology overlap each other, just as, in the annual budget cycle, the planning for each fiscal year takes more than a year. In 1961 we had on hand major weapon systems, conceived in the middle 1940's and developed in the late '40's or early '50's, that became conceptually obsolete (in the sense that superior systems were already planned) before their development was completed but that will not be phased out for several years yet. We also have major weapon systems that presumably could now have quite different characteristics had arms-control criteria been imposed on them in the development stage. But these systems would now be difficult to accommodate to new restrictions, because development has ceased, designs have been frozen, and production is under way. And very little organized knowledge is available for evaluation of the impact of specific restrictions on the military establishment as a whole.

The potential speed with which arms controls could be introduced evidently depends on the speed with which the necessary adaptation of weapon systems and personnel could be accomplished. The technical and economic inertia of weapon programs is a potent parameter in determining the potential pace at which arms controls could be introduced.

It is apparent, therefore, that case studies leading to generalizations about the life histories of typical weapon systems and their impact on military establishment might produce not only better appreciation of military programs themselves, but also might help to determine some of the constraints on arms control. This is not just a matter of the technology of hardware production. It involves internal organization, training, and doctrine; it is sociology as well as industrial economics.

It is hard to exaggerate the importance of this subject for any possible arms agreement that would seek to standardize weapons among adversaries, to freeze technology, to encourage qualitative developments like "second-strike-only" forces, or to ban particular weapons and weapon

characteristics that are involved in weapon systems already planned or in procurement. Clearly it would be most valuable to concentrate research en the weapon systems most likely to be involved in arms controls; but, pending a better identification of the contents of possible arms agreements, it would be helpful to have a better understanding of typical time schedules in the conception, development, procurement, and obsolescence of weapon systems and personnel.*

The Role of Demand in Technological Trends

Much discussion of the arms race is oriented towards the inexorable trends of technology and the difficulties of suppressing mischievous application of new knowledge. At the same time, much discussion of the role of science in modern life is oriented toward the influence of military demand in determining the relative speeds of development of particular lines of technology. It is an important question, and an interesting one, to determine to what extent military technology is autonomous, ineluctable, and insuppressible and to what extent it reflects the explicit or implicit values and prejudices of military and political planners.

Science and technology have a certain impetus of their own. New technical ideas depend on luck, on individual curiosity, or intuition, and on a disorganized consensus as to the directions of development that promise the greatest payoffs. But even the conscious efforts to direct trends in science and technology are based on imperfect notions of the ultimate values of particular developments and of the potential demands for particular new ideas.

Nevertheless, some scientific discoveries and technical developments are more highly rewarded than others, and some are more eagerly sought. and encouraged than others. In one way or another, money, time, and attention get allocated to a selection of possible routes of development out of an almost infinite array of potential development paths.

Some examples may illustrate the point. Nearly all who have (or respond to) a military interest in the development of aircraft have a strong bias in favor of speed and only a moderate interest in endurance. In two decades, we have progressed from less than half to triple the speed of sound. Speed is obviously desirable, and improvement was predictable. We have also increased the range of bomber aircraft. This, one may conjecture, was also predictable on the basis of science. Had the United States been located two thousand miles nearer Europe and Asia, extensions in range might have been less readily forthcoming. But what we strikingly lack is an increase in the ability of aircraft to remain aloft. Until very recently, recognized military missions put no great premium on the sheer airborne endurance of aircraft. Newspapers give virtually no coverage to proposals for increasing endurance. Instead, it is the Mach 3 B-70 that gets the most attention from the Air Force, the Congress, and the press. The conception of a chemically-fueled long-range aircraft receives little attention in the Air Force and less in the newspapers.

---

*Lead-times in arms-control systems are important too, of course, but less likely to be neglected in disarmament planning.

.On the other hand, nuclear-powered aircraft have re͏ ͏ͅ.ͅ·d attention.
Long endurance is a characteristic associated with nucle ͏ ͏ n͏ wer. One
suspects that the glamour of exotic propulsion helped to attract support for
this concept since chemically-fueled aircraft that might stay aloft several
days have so few admirers. (Of course, interest in nuclear fuel may lead
to an interest in endurance for its own sake apart from the fuel that pro-
duced it.)

The point here is not that long-endurance aircraft are valuable but
that if they are valuable they probably receive unduly small emphasis in
military planning.* If so, their low priority may be a reflection of biased
supply and demand in the development of technology. To a large extent,
the character of modern weapons may reflect not the accidents of nature
and the inexorable progress of scientific curiosity, but rather the value
judgments of military planners.

This conclusion has relevance to one problem of arms control. It is
often pointed out that nearly all important strategic weapon systems em-
body an impressive counterforce capability. This fact is an embarrass-
ment to those who have proposed that both sides might agree to concen-
trate on second-strike weapons and eschew first-strike weapons. It is
difficult to identify really good second-strike weapon systems that do not
embody, or that cannot readily be made to embody, a persuasive first-
strike capability. Accepting as historical fact that most strategic weapon
systems in existence or on the drawing boards have a fairly good first-
strike capability, one can still inquire whether we are not dealing with a
biased sample. Perhaps this is because those weapon systems that lack
a first-strike capability are unlikely to receive encouragement. Perhaps
the selection process has been one in which good second-strike weapons
are developed and procured only if they appeal to the kind of judgment that,
implicitly if not explicitly, puts a high value on counterforce capabilities
and low value on pure retaliation. (The fast versus the long-endurance
aircraft is pertinent here.)

What I am proposing is the hypothesis that our generalizations about
military technology, and about the characteristics that weapon systems
"necessarily" acquire, are based on a biased sample--on observation
of the weapon systems that have in fact been developed. If military
technology is responsive to the value judgments of military planners and
if these planners have emphasized particular capabilities and have tended
to eschew or to discourage other weapon characteristics, the range of
possible characteristics that might have been developed was greater than
what we are acquainted with. And the scope for shifting emphasis in
weapon characteristics is greater than the history of actual weapon de-
velopment would suggest.

This is only an hypothesis, but it deserves to be examined. It can be
examined. A contrast of the long-range chemically powered aircraft with
the B-70 would typify the kind of retrospective comparison that might be

---

*In addition to the possible value of long-endurance characteristics
for airborne alert and other military operations, there may be a number
of arms-control inspection uses, either in peacetime or at .he termination
of war.

made. (Nuclear vs. conventional arms for limited war would be another
interesting case.) It should be possible to look back over the history of
weapon development of the last ten years and proposals to see which kinds
of development proposals withered for lack of appreciation and which kinds
were pushed with money and encouragement. It should be possible to iden-
tify areas of technological backwardness--projects that could have borne
fruits by now had attention been given them--and to see to what extent
these developments lacked appropriations and attention. If arms control
is going to depend on the development of weapon characteristics different
from those that might "automatically" ensue, it is important to know
whether or not a shift in values, in the character of the demand for weapon
characteristics, can in fact much influence the character of weapons to come.

CONCLUSION

May I emphasize again that the topics sketched in this study are not a
screened highest-priority group of claimants for support. They have been
selected to suggest and to illustrate the kinds of topics that need to be
opened up or explored further; they are samples, not a preferred panel of
candidates.

Two important areas have been omitted altogether. One is the eco-
nomic consequences of arms, reduction, both problems and opportunities.
I have neglected it for two reasons. First, the economic problems and
dislocations would, for the most part, be familiar rather than unique and
would not require fundamentally new ideas or techniques.* Second, in my
judgment, the likelihood and magnitude of arms-budget reductions have
been commonly exaggerated even assuming progress in arms control. The
reason is not so much in the modesty of the programs that may be adopted
but rather in the possibility that they may be expensive to adapt to (and
perhaps expensive to monitor).**

The other omission is a profound one. I have not, in this study, rec-
ommended examination of the purposes and probable accomplishments of
arms control, the criteria by which proposals should be judged, the merits
and demerits of specific agreements that might be reached, or the relation
of arms control to military policy and military programs. I have not, in
other words, gone to the center of the subject!

There is only one excuse for this, and it is not that we have mastered
the center and need to move now toward the periphery. It is rather that an
attempt to organize the outstanding strategic issues would have dominated
this study and turned it into one on strategy.

In thirteen topical areas discussed above, are a number of positive
suggestions for social science research on arms control. These sugges-
tions may intrigue some of those social scientists who wish to contribute

---

*Of course, existing economic knowledge has to be focused on the par-
ticular problem of arms-budget changes. The Research Program on Eco-
nomic Adjustments to Disarmament, of which Professor Emile Benoit of
Columbia University is the director, is presently attempting just that.
**See Schelling (37) and Schelling and Halperin (41, pages 120-127).

258   Schelling

to national security. Research on many, perhaps most, of the suggested
topics would contribute also to the basic fund of social science knowledge
needed by policy-makers in the solution of many international problems.

REFERENCES

1. Ardant du Picq, Charles J. Battle Studies. New York: Macmillan
   Co., 1921.

2. Barnet, Richard J. "Russia, China, and the World," Problems of
   Communism, 10 (1961), 32-37.

3. _____. Who Wants Disarmament? Boston: Beacon Press,
   1960.

4. Bechhoefer, Bernard G. "Negotiating with the Soviet Union," in
   Brennan (6, pp. 269-281).

5. Boulding, Kenneth E. Conflict and Defense. New York: Harper and
   Bros., 1962.

6. Brennan, Donald G. (ed.). Arms Control, Disarmament, and National
   Security. New York: George Braziller, 1961. (Most of the essays
   contained in this volume appeared in the Fall 1960, special issue of
   Daedalus on "Arms Control.")

7. Bull, Hedley. The Control of the Arms Race. New York: Praeger for
   The Institute for Strategic Studies, 1961.

8. Burns, Arthur L. "A Graphical Approach to Some Problems of the
   Arms Race," Journal of Conflict Resolution, 3 (1959), 326-342.

9. _____. "The Rationale of Catalytic War," Research Monograph
   3. Princeton, N.J.: Princeton University, Center of International
   Studies, 1959.

10. Crozier, Brian. The Rebels: A Study of Post-War Insurrection.
    Boston: Beacon Press, 1960.

11. Dennett, Raymond, and Johnson, Joseph E. (eds.). Negotiating with he
    Russians. Boston: World Peace Foundation, 1951.

12. Dinerstein, Herbert S. War and the Soviet Union. New York:
    Frederick A. Praeger, 1959.

13. Douglas, Ann "The Peaceful Settlement of Industrial and Intergroup
    Disputes," Journal of Conflict Resolution, 1 (1957), 69-81.

14. Dresher, Melvin. Games of Strategy: Theory and Applications.
    Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1961.

REFERENCES (Continued)

15. Engel, J. H. "A Verification of Lanchester's Law," Journal of the Operations Research Society of America, 2 (1954), 163-171.

16. Finkelstein, Lawrence S. "The Uses of Reciprocal Inspection," in Disarmament: Its Politics and Economics, ed. Seymour Melman. (Special issue of Daedalus.) (Pp. 82-98.) Boston, Mass.: The American Academy of Arts and Sciences, December 1962. (Forthcoming in book form.)

17. Fisher, Roger. "Constructing Rules that Affect Governments," in Brennan (6, pp. 56-67).

18. Halperin, Morton H. Limited War in the Nuclear Age. New York: John Wiley & Sons, 1963 (forthcoming).

19. Henkin, Louis (ed.). Arms Control: Issues for the Public. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1961.

20. Iklé, Fred C. "After Detection, What?" Foreign Affairs, 39 (1961), 208-220.

21. _____. Alternative Approaches to the International Organization of Disarmament. (RAND report no. R-391-ARPA.) Santa Monica, Calif.: RAND Corporation, February 1962.

22. Jessup, Philip C., and Taubenfeld, Howard J. Controls for Outer Space and the Antarctic Analogy. New York: Columbia University Press, 1959.

23. Kahn, Herman. On Thermonuclear War. Princeton, N.J.: Princeton University Press, 1960.

24. Kent, Glenn A. On the Interaction of Opposing Forces under Possible Arms Agreements. (Occasional Papers in International Affairs no. 5.) Cambridge, Mass.: Harvard University, Center for International Affairs, February 1963.

25. Knorr, Klaus (ed.). NATO and American Security. Princeton, N.J.: Princeton University Press, 1959.

26. Lanchester, F. W. "Mathematics in Warfare," in The World of Mathematics, ed. James R. Newman. (IV, pp. 2135-2157.) New York: Simon and Schuster, 1956.

27. Lefever, Ernest W. (ed.). Arms and Arms Control. New York: Frederick A. Praeger, 1962.

28. Lerner, Daniel, and Gorden, Morton. European Leaders Look at World Security. (C/60-7.) Cambridge, Mass.: Massachusetts Institute of Technology, Center for International Studies, 1960.

29. Mackintosh, Malcolm, and Willetts, Harry. "Arms Control and the Soviet National Interest," in Henkin (19, pp. 141-173).

REFERENCES (Continued)

30. McNeil, Elton B. "Personal Hostility and International Aggression," Journal of Conflict Resolution, 5 (1961), 279-290.

31. Morse, Philip M., and Kimball, George E. Methods of Operations Research. New York: John Wiley & Sons, 1951.

32. Morton, Louis. "War Plan ORANGE: Evolution of a Strategy," World Politics, 11 (1959), 221-250.

33. Nogee, Joseph L. "The Diplomacy of Disarmament," International Conciliation, No. 526 (1960), 235-303.

34. _____ . Soviet Policy Toward International Control of Atomic Energy. Notre Dame, Ind.: University of Notre Dame Press, 1961.

35. Rapoport, Anatol. Fights, Games and Debates. Ann Arbor: University of Michigan Press, 1960.

36. Richardson, Lewis F. Arms and Insecurity. Pittsburgh, Pa.: The Boxwood Press, 1960. (Published posthumously.)

37. Schelling, Thomas C. "Arms Control Will Not Cut Defense Costs," Harvard Business Review, 39 (1961), 6-14, 156, 158.

38. _____ . "The Role of Deterrence in Total Disarmament," Foreign Affairs, 40 (1962), 392-406.

39. _____ . The Strategy of Conflict. Cambridge, Mass.: Harvard University Press, 1960.

40. _____ . "Surprise Attack and Disarmament," in Knorr (25, pp. 176-208). (Reprinted, abridged, in Schelling, 39, pp. 230-254).

41. _____ , and Halperin, Morton H. Strategy and Arms Control. New York: The Twentieth Century Fund, 1961.

42. State Department, U.S. Geneva Conference on the Discontinuance of Nuclear Weapon Tests: History and Analysis of Negotiations. (U.S. Department of State publication no. 7258; Disarmament Series 4.) Washington, D.C.: Government Printing Office, October 1961.

43. Taubenfeld, Howard J. "A Treaty for Antarctica," International Conciliation, No. 531 (1961), 245-322.

44. Teller, Edward. "The Feasibility of Arms Control and the Principle of Openness," in Brennan (6, pp. 122-137).

45. Weiss, Herbert K. "Lanchester Type Models of Warfare," Proceedings of the First International Conference on Operations Research. Baltimore, Md.: The Conference, 1957.

46. Wohlstetter, Albert. "The Delicate Balance of Terror," Foreign Affairs, 37 (1959), 211-234.

REFERENCES (Continued)

47. Wolfers, Arnold. Discord and Collaboration: Essays on Internat
    Politics. Baltimore, Md.: The Johns Hopkins Press, 1962.

48. _____(ed.). East-West Negotiations. Washington, D.C.:
    Washington Center of Foreign Policy Research, 1958.

49. Zoppo, Ciro E. The Issue of Nuclear Test Cessation at the Londc
    Disarmament Conference of 1957: A Study in East-West Negotiat
    (RAND memorandum no. RM-2821-ARPA.) Santa Monica, Calif.:
    RAND Corporation, September 1961.